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IN

BRITISH LEPIDOPTERA.

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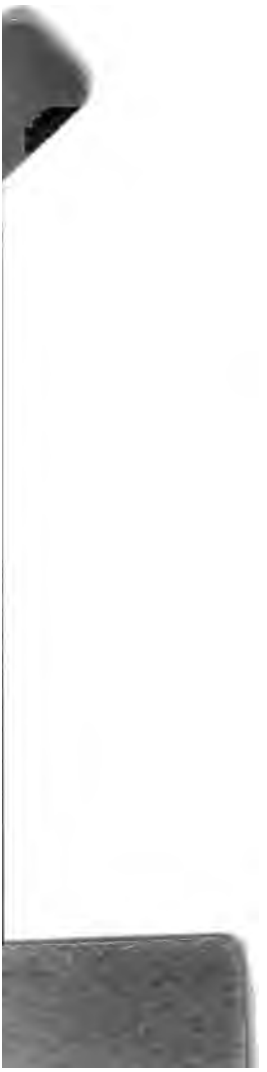

MELANISM AND MELANOCHROISM

IN

BRITISH LEPIDOPTERA

BY

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MELANISM AND MELANOCHROISM IN BRITISH LEPIDOPTERA.

"MELANISM" in Lepidoptera is a subject which has now and again been brought before the entomological public, and discussions as to its origin and distribution have occasionally taken place in the British entomological journals. In these discussions, the subject was generally treated from an insular point of view, until Mr. Dobrée (*Entom.* xx., pp. 25–28) endeavoured, by comparing our melanic forms with the forms of the same species obtained on the Continent, to correlate the facts obtained, and to show the real relation that not only our melanic, but also our ordinary forms bore to the Palæarctic lepidopterous fauna, and to deduce reasons for the melanism so prevalent in our British forms. In dealing with melanism it is advisable in the first place to bear in mind what we include in that term. Melanism, properly speaking, refers only to an increase in the quantity of actually *black* markings at the expense of any other colour. It has not been customary, however, to base or confine our arguments to this restricted view of the subject, but any darkening of the ground colour, whether black or not, has been included in a general way in the term. However, as this was not strictly correct, Dr. F. Buchanan White (*Ent. Mo. Mag.*, vol. xiii., p. 145) suggested for this general darkening of the ground colour the term "melanochroism," whilst the opposite phase of variation, the development of a paler pigment at the expense of a darker, he called "leucochroism." Thus, *Amphidasys betularia* var. *doubledayaria*, is a case of true "melanism." The development of buff and yellow coloration, instead of white, in *Arctia menthastri* is a case of "melanochroism." It will be seen that most of the cases referred to in the discussions on "melanism" are really not true melanic forms, but really melanochroic forms, because

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the species are but rarely suffused actually with black, although there may be a great deepening in the ground colour, the depth of the colour being, however, frequently increased by a colour other than black. A good example of "leucochroism" is that of *Arctia plantaginis* var. *hospita*. In this variety the orange of both the anterior and posterior wings is replaced by white.

I believe Mr. Cockerell was the first entomologist who attempted directly to connect certain forms of melanic variation with an excess of moisture, although in the *Ent. Mo. Mag.*, vol. xiii., p. 148, Dr. Buchanan White stated most explicitly that he believed the exciting cause of melanism was to be looked for in certain meteorological conditions. However, a paper that Mr. Cockerell read before the members of the South London Ent. and Nat. History Society (*Transactions S.L.E. and N.H.S.*, 1887, pp. 103, 104) caused me first to give particular attention to it. It struck me at once that a great deal of the melanism and melanochroism of our Islands could be better explained by this theory—in combination with "natural selection,"—than by any other that had been presented to us. Our greatest authority on Continental NOCTUÆ, Mr. N. F. Dobrée, wrote an article (*Entom.* xx., pp. 25–28), previously referred to, disproving the general notion that melanism was characteristic of high latitudes, and pointing out the following facts:—(1). That melanism scarcely ever occurred in such latitudes. (2). That at any latitude, dry open areas produced more brightly and clearly marked forms of lepidoptera, and (3). That the North of Europe produced, practically, no melanic forms, neither did the South, but that the melanism of the Continent was confined almost entirely to certain Alpine districts. This was followed up (*Entom.* xx., pp. 58, 59) by a short article from Mr. Cockerell, who, of necessity accepting Mr. Dobrée's facts, disagreed with that gentleman's deductions and conclusions, and suggested, that the areas where melanism was prevalent were more or less humid, and that humidity had probably more to do with melanism than food or any other cause. Since I have devoted my attention to the matter, the information I have been able to gather helps to confirm Mr. Cockerell's view.

Taking into account the physical geography of Europe, what do we find are its broad, general, meteorological and climatic characters? (1). That the great central plain, comprising Russia, Northern Germany, Holland, Belgium, and

Northern France, is swept by the prevalent east winds, which, having traversed Siberia, are totally devoid of moisture, and that consequently this area has a comparatively small rainfall, and has exceptionally dry air.

(2). That the great central mountain chain, the Alpine-Carpathian range, running through Europe, east and west, has a much heavier rainfall and moister atmosphere, due to condensation, than the great central plain.

(3). That the district south of the central mountain chain, which may be said to border the Mediterranean littoral, is subject occasionally to heavy rainfalls, but has an atmosphere remarkably clear.

Here, then, we have two great areas, one north and the other south of the great central chain, where the air is comparatively dry and clear, and in these areas we find little or no traces of melanochroism, or darkening of colour; but in the more humid mountainous districts we get forms closely resembling our own melanic (but not extreme) forms, as the term "alpine," so frequently applied to mountain forms from the Continent, and to the ordinary forms obtained in this country, testifies.

I would now call attention to the general facts mentioned by Mr. Dobrée (*Entom.* xx., pp. 25–28), where, after citing numerous examples, he states in his excellent paper:—"This at once fixes the fact that melanism is of purely British occurrence, thereby upsetting the assumption of our theorists, that darker colouring and smaller size is a feature of high latitudes," etc. "If we once more return to our starting point—in Central and Southern Germany—and turn our eyes southwards, it will be found that the prominent features of lighter colour and fainter markings of high northern latitudes again become apparent," etc. "The absence of melanism in Continental Europe is not, however, without its exceptions, for in the high Alps of Switzerland, Styria, and Carniola it occasionally appears," etc. "The inference can be drawn, that melanism is primarily due to the peculiar geographical position of these Islands."

Before dealing with the peculiarities of the meteorology and physical geography of the British Islands, I should like to quote a remark made by Dr. White (*Ent. Mo. Mag.* iv., p. 248), where he speaks of the peculiarity of the fauna and flora of Western Scotland, although not then referring to variation. He writes:—"No one who has studied the

'*Manual*' can have failed to have noticed a great similarity between the fauna in the Lake District of England and that of Rannoch in Scotland. In the Lake District we have a similar elevated region of lakes and mountains, with a like western situation. . . . . Another fact worth noticing about Rannoch is, that while it seems to appropriate to itself most of the Alpine and boreal insects, yet it possesses but few (and these the commonest) Alpine plants; while the neighbouring district of Breadalbane, lying immediately to the south, has more Alpine and boreal plants than any other place in Britain, and yet but few Alpine and boreal insects. Breadalbane, however, has higher mountains and less wood, which may perhaps account for the difference." I would point out how conclusively in this extract Dr. White proves that food has nothing to do with the peculiarity of the Rannoch, or, in a wider sense, the Alpine fauna. Generalising, we find Dr. White again writing:—"Botanists are aware that the plants of the west coast are less brilliantly coloured than those of the east; and I think that it is in the west rather than the north that melanochroism in British insects may best be studied."

The excellence of these observers is beyond question, and the extracts quoted will, I believe, help me to make clear my argument.

I have already given a brief summary of the general meteorological and climatic conditions of the Continent, and shown the limited extent of melanism existing there. I would now turn to our own country and examine its peculiar climatic and meteorological conditions, and compare them with those of the Continent. The following appear to be the chief points to which attention should be directed:—

(1). The first thing that we must notice is the fact that our climate is strictly insular, with an average temperature of 40° to 60° F., with very few extremes of either excessive heat or cold. On the Continent great extremes are registered.[1]

(2). The meteorological conditions of our Islands are governed almost entirely by the influence of the Gulf Stream.

[1] In Keith Johnston's *Physical Geography*, pp. 52 and 53, we find:—"Our climate in the British Isles is a decidedly maritime one; its average temperature ranging from about 40° F. to 60° F. In Central Asia, however, in the same latitude, and at the same height above the sea, the average temperature ranges from about 0° F. in winter to about 70° F. in summer. The temperature of the British Isles surrounded by the sea thus varies only 20° on an average during the year, but that of the centre of the Continent in the same latitude changes to the extent of 70°."

This Stream, which is a warm current with an average temperature of 30° F. above the surrounding ocean, is a branch of the Atlantic equatorial current. After passing round the Gulf of Mexico, the Gulf Stream runs round Florida into the Atlantic Ocean, skirts the eastern coast of the United States for some distance, and then strikes off in a north-westerly direction across the Atlantic towards Britain and Scandinavia. As I have stated, its temperature is generally much higher than the surrounding ocean, hence there is always a great deal of vapour rising from it. This vapour, therefore, affects the atmosphere over this current, and renders it moist and humid.

(3). The prevailing winds in Britain are from the south-west. These winds blow over the Gulf Stream; hence in their course they become exceedingly humid, and laden with moisture before they strike our western shores.

(4). The easterly winds, which blow from the icy plains of Siberia, are so extremely cold that almost all the moisture has been condensed before they cross the Continent of Europe. The North Sea is too narrow to modify them to any very great extent; hence they strike on our shores as dry, piercing winds, and thus affect our eastern coast in a contrary direction to the prevailing south-west winds, and so help to counteract and modify the influence of the latter.

Now let us combine Nos. 2 and 3 above. The moisture which has been obtained by the south-west winds in their passage across the Gulf Stream is driven by them over our western shores. The air has to rise to cross our hills and mountains, and falls as rain. As the wind comes on, it loses more and more of its moisture, until, by the time it reaches our eastern shores, the quantity of moisture is comparatively small. Hence we find that the west coasts of our Islands have excessive annual rainfalls—sometimes from 70 to 80 inches—whilst the average of the south coast is less than 20. Not only is the rainfall of our west coasts heavy, but the air is much more humid than that of the east.

But it must not be supposed that the climate of the east coast is not modified at all by the Gulf Stream. The humidity of the air on the east coast is much greater than on the Continent, where the prevailing winds are east or north-east, and even the easterly winds are slightly less dry than when they cross the Continent, as the North Sea, narrow as it is, has some little effect in modifying them in this way.

Summarising the result, we find—(1). That North and North-western Scotland and Ireland are the most humid districts in Britain; Eastern and Southern Scotland, North-western England and Wales following; the Midlands next; and the South and Eastern parts of England least humid of all. (2). That our least humid areas are much more humid than areas in corresponding latitudes on the Continent, and all high-lying areas have a greater rainfall than low-lying areas in the same districts.

I would now direct attention to a few facts relative to the distribution of melanism in this country.

Mr. Percy Russ has shown us that a very great number of remarkable cases occur on the west coast of Ireland. At Sligo a very large number of species are melanic, and probably this is more noticeable among the NOCTUÆ than any other group. The greater part of Ireland is comparatively unworked. Collectors have from time to time stayed at different places, and have at various times compiled lists of the different species that have come under their notice, but this method fails almost entirely in giving us scientific results. The energy of our professional collectors has opened up to us the wonderful melanic variation so frequent in the Hebrides and other islands of our western shores, and also those from the Northern Shetlands and Orkneys, where moisture reigns supreme. Professionals and amateurs alike have recorded the variation and general tendency to melanism in the Rannoch district. Aberdeen, Perth, Dundee, and Glasgow have each their own body of active workers, and Lancashire and Yorkshire have long boasted a large proportion of the leading lepidopterists of the United Kingdom, and these have given us an immense mass of information relative to the melanic variation in their various districts. Most of these districts have an exceedingly humid atmosphere, and they produce a greater or less number of melanic forms, but the most intense forms are undoubtedly from the west coast of Ireland, the Scotch Islands and Highlands, where the humidity of the atmosphere is most excessive. There is a great deal of difference between the degree of melanism on the east and west coasts of Scotland. Repeatedly we hear that "the fauna of the east coast is less Alpine than that of the west." With regard to the distribution of melanic forms in Britain, I will again quote Dr. White. He writes:—"That there is frequently a difference between *South* England and

Highland examples of the same species, will be admitted, I think, by every one; and that this variation is, in the majority of cases, in the direction of melanism has been generally taken for granted. That it really is so, the following brief analysis[1] of the Highland *Lepidoptera* will show. In it I have compared the majority of the Macro-Lepidoptera of Scotland north of the Tay, with South England (and a few cases with South European) specimens of the same species; for it must be remembered that North and North-western English specimens frequently exhibit the same or even a greater tendency to melanism than the Highland specimens" (*Ent. Mo. Mag.*, vol. xiii., p. 146). Mr. Birchall also in the same volume, p. 131, after referring to the melanism of the Highlands of Scotland, writes:—"It must further be noted that melanic varieties of Lepidoptera occur very commonly in Ireland, the Isle of Man, Durham, South Lancashire, and the West Riding of Yorkshire, under various conditions of climate and soil."

Now to turn from effects to probable causes.

As I have pointed out above, the western coasts of our Islands are more humid than the eastern, and the Gulf Stream exerts its greatest influence on the western shores of Ireland, the north-western shores of England and Scotland and the Northern Islands; and here, following out the connection of humidity and melanism, we find that, in those districts, the humidity[2] of which is so striking, we get our most pronounced forms of melanism. The blackness and intensity of colour of certain forms of lepidoptera, especially among the NOCTUÆ from Sligo, Rannoch, the Hebrides, Orkneys and Shetlands are most striking and remarkable, and here are our most humid districts. In a lesser degree the lepidoptera of Eastern and Southern Scotland and North and North-western England show general melanism, but rather less intensity of development in some particular species. Then comes the melanism of Wales and the Midlands; and lastly, the ordinary melanism found in the species of our south and south-eastern districts as compared with the forms of the same species obtained on the Continent. In other words, we find a certain gradation, corresponding, in a general way, with the gradation in humidity.

[1] Dr. White (*Ent. Mo. Mag.*, vol. xiii., pp. 145–147) gives a long list of examples, as also does Dr. Birchall (vol. xiii., p. 133 of the same journal).

[2] *i.e.* "Humidity" apart from heavy rainfall.

With regard to the insects on our south and south-eastern coasts, it is rather remarkable that the difference between them and those of the corresponding coasts of the Continent should be so very marked; but it is so, and I am not surprised that Mr. Dobrée (*Entom.* xx., p. 28) should have come to the conclusion that "the inference can be drawn that the melanism is primarily due to the particular geographical position of these islands." To complete the connection between the relation which I have tried to prove exists between "humidity" and "melanism," it would be interesting to know whether the influence of the Gulf Stream makes itself felt sufficiently to produce melanic forms on the western coast of the Scandinavian Peninsula. I know nothing of the fauna of this district; but if melanism occurs there, it would do much to strengthen the view taken.

That this melanism exists in Scandinavia the following note from Professor Schöyen, Christiania, will show:—"The tendency to melanism in our lepidoptera is certainly, as a rule, more apparent on the western shores than in the eastern districts of the country, even at a less height above the sea-level; in some cases this is as marked as in the mountainous tracts,—*e. g. Cidaria flavicinctata* occurs at the town Molde in Romsdale Amt, close to the sea-shore, much darker than at Dovrefjeld, about 2,000–3,000 feet above the sea; *Agrotis candelarum* occurs at Bergen as dark as on the Jotünfjelds (var. *jotunensis*, mihi). But in the majority of species, the darker colour is generally found, however, either at a greater height above the sea, viz., on the mountains, or in a more northern latitude. Yet this may also be chiefly caused by the more humid condition of the atmosphere, for the mountains are generally covered with masses of cloud, making the atmosphere much more humid than in the valleys and lowlands, and in the northern latitudes, large quantities of rainfall and fog and mist are prevalent. Under these circumstances the grey colour of many moths will generally be more or less darkened" (*in litt.* 18/3/90). I need not point out how valuable this confirmation of my theory is.

The melanism of Lancashire and Yorkshire deserves to be considered separately. Mr. Dobrée (*Entom.* xx., p. 28), considered the melanism of Yorkshire as "exceptional, and not affecting the general question." Mr. Cockerell, on the other hand (*Entom.* xx., pp. 58, 59), considered that the "melanism of Yorkshire would follow the general rule, and that the

melanism of Yorkshire would be found coupled with a greater degree of humidity in those localities where it occurs."

I am inclined to agree partially with both these statements. The melanism of Yorkshire is "exceptional," as many places apparently more humid (because they have a greater rainfall) do not produce such extreme cases of melanism, while there is no doubt that the humidity of the atmosphere in Yorkshire is greater than its rainfall would apparently lead one to suppose it to be. Hence I consider Mr. Dobrée correct in considering it "exceptional, and not affecting the general question;" while there is no doubt that Mr. Cockerell's suggestion is in the main also correct, and that the atmosphere is sufficiently humid to account for a great deal of the melanism which occurs there. But I agree with Mr. Dobrée in considering the melanism of these counties exceptional, for a reason which will appear directly. I have been informed by Mr. Porritt, that "the rainfall of West Yorkshire is about 33 inches, and in Lancashire much the same"; but "the rainfall in Cumberland is much greater; and yet there are not such striking melanic forms, although probably the soil retains the moisture more in Yorkshire" (*in litt.*). If this be so, although the rainfall of Yorkshire is less heavy, the general humidity of the air would probably be greater. But some years ago (1877) there was a discussion (*Entom.*, vol. x., pp. 92–96), in which some of our leading lepidopterists took part. With all due respect to the opinion of the others who took part in the discussion, I think Dr. Buchanan White and Mr. Nicholas Cooke (so far as the discussion in the *Entomologist* was concerned) took up the two essentially strong sides of the question. Taking it up from a purely insular point of view, however, neither of these gentlemen appear to have come to any strong or satisfactory conclusions. Dr. White took up the view of "natural selection" and "protection." Mr. Cooke went against the view of "natural selection" (which he seems to have made up his mind was nothing more than a modified form of "sexual selection," although I doubt whether anyone saw it in the light he presented it), and argued that melanism was caused by fumes, etc., acting chemically on the food, and physiologically on the larva. I cannot myself imagine smoke or fumes, by means of food, affecting the organisation of any animal in such a way as to produce melanism, and I doubt whether melanism can be produced or ever has been produced in this way; but combining Dr. White's and Mr. Cooke's

theories, I can understand that smoke and fumes would darken the objects in the localities where they are poured out in such excessive quantities, and then by Dr. White's argument of "natural selection" and "protection," I can understand how melanic forms are perpetuated and intensified. Mr. Birchall (*Ent. Mo. Mag.*, vol. xiii., p. 131) took up the view of deficient amount of sunlight owing to the "excessive moisture," as being the exciting cause in Ireland, Scotland and the Isle of Man, and deficient sunlight due to "exhalations from furnaces and chemical works" in manufacturing districts, and further states that "the sun is obscured by the clouds of coal smoke, and the vegetation defiled and destroyed by the deposits of soot." He then argued that having obtained dark forms, "the dark forms have greater strength of constitution, etc., and that the light forms become eliminated." The larva of *Agrotis ripæ* lives underground all day, pupates underground, and yet the imago is often pale ochreous or silvery white in certain localities, never black; yet as far as this argument goes, this species (and all underground feeding larvæ, etc.) should produce specimens which ought to be specially black. Of course, the white colour is in my opinion, due to the pale colour of the sand in the localities where these forms are found, and in which the imago hides by day. But Dr. Chapman (*Ent. Mo. Mag.*, vol. xxv., p. 40), by some observations which he had made, and which he there related, has given us another clue, and opened up a new phase of the question. He suggested that a darkening of the surfaces of trees and similar objects was produced by rain, and hence the darkening of many species might be due to this cause for the purpose of protection. But this appears only to touch the fringe of the question as the surfaces are not made *permanently* darker by rain. How does rain permanently darken the tree-trunks and other objects on which insects rest? It is well known to the most elementary student of chemistry that rainwater rarely occurs pure, and that in large towns the quantity of impurities is very great. The vast quantity of smoke, gases, fumes, etc., in manufacturing towns, brought down by rain, is scarcely credible, and it is from these impurities I consider the permanent darkening comes. When the water evaporates, the solid matter is left behind, and as a result the impurities are left to darken the surfaces to which they have been carried by the rainwater. The theories of "natural selection" and "protection" now apply in their fullest

sense, the insects become darkened, "hereditary tendency" perpetuating and intensifying the melanism. I believe from this (and it appears to be a fair deduction), that Lancashire and Yorkshire melanism is the result of the combined action of the "smoke," etc., *plus* humidity, and that the intensity of Yorkshire and Lancashire melanism produced by humidity and smoke, is intensified by "natural selection" and "hereditary tendency." As example I would cite the species *Amphidasys betularia*, *Tephrosia biundularia*, *Boarmia rhomboidaria*, *B. repandata*, *Diurnæa fagella* and *Hibernia marginaria* (*progemmaria*), which, occurring on trees, fences and similar objects would thus be affected. Taking this view, I consider the melanism of these counties "extraordinary;" I also consider it due to "humidity." The melanism of Staffordshire, Derby, and other localities of a similar character, I consider, is produced much in the same way, and the protective melanism of London lepidoptera and of other large towns I would refer to the same cause. Probably no better instance of this kind occurs among London lepidoptera than *Eupithecia rectangulata* var. *nigrosericeata* (another species which in the imago state frequents tree-trunks).

But there is no doubt that another local condition, at least, is brought into play in aiding "natural selection" in its work. I refer to the presence of large wooded tracts in districts where this artificial (so to speak) darkening of surfaces is occurring. It is well known that large arid tracts of land in different parts of the world have been planted with trees to increase the water supply of such districts, and it is as well known that the action of these trees on the atmosphere is such, that vast quantities of vapour are condensed from it, and the atmosphere in such a district becomes charged with moisture and very different to its previous dry condition. It is also well known that all large wooded areas have a moister atmosphere than areas in immediate contact with them which are open in character. Probably no trees have a tendency to hold so much moisture as the different kinds of fir trees. I have long noticed this tendency, and there is no doubt that the presence of moisture in such localities, helps to make the natural dark colour of these trees still darker; add to this that fir trees, by means of their foliage, shut out an immense proportion of light, and we can readily understand that "natural selection," would leave, to a great extent, only the darker insects in such situations. I find it an actual fact in the south of England, that such localities

give us a greater proportion of darker specimens of many species that naturally rest on tree-trunks, than do comparatively open woods of more mixed growth. If we add to these natural conditions the presence of chemical fumes, smoke, and other atmospheric impurities which are specially characteristic of large manufacturing districts, we can see how intensified the tendency to melanism would become in such districts. Only those insects that become positively black, would have in many places the slightest chance of escape, and "natural selection," by weeding out the paler specimens, would leave only the darker, and "hereditary tendency" would play its part in the intensification of the colour of the species. I would now cite examples which I think will tend to prove this view.

Thanks to the generosity of Mr. Sheldon and Mr. John Hill of Derby, most of us I suppose, who are interested in this subject, possess specimens of *Tephrosia biundularia* from this locality. Those I had some years ago from Mr. Sheldon are dark, but comparatively pale compared with some sent me a few years later by Mr. Hill. Some time ago Mr. Sheldon, who is one of our most observant lepidopterists, told me what he considered the history of the melanism of *Tephrosia biundularia* in the neighbourhood of Derby,[1] and for the purpose of this paper, I wrote to Mr. Sheldon, asking him to give me the particulars. I now quote his statement, which is as follows:—"Some 12 or 13 years ago, I found that *biundularia* occurred freely in a wood near Derby. The growth was composed of spruce, and Scotch firs, larch, oak, etc., and would be then some twenty years of age. The *biundularia* were of all shades, from the light southern type to a form about as dark as typical *T. punctulata*. A large percentage, 50 per cent., would be the light form. This was from 1876–1879. In 1880 I left the district, and did not do any more collecting in it till 1885, when I found a difference in the *biundularia;* not only were the extreme dark forms darker than any I had previously noticed, but the percentage of pale ones was much less, not more than about 15 to 20 per cent. of the whole. I have not done any collecting there since, but I am told by a resident collector that it is now difficult to find the light type, and so the darkening process appears to be going on." With respect to the cause of this, Mr. Sheldon writes:—"I consider it to result from two reasons:—(1). The gradual growth of the trees makes the wood darker than formerly. (2). The dark

[1] Vide *Ent. Mo. Mag.*, vol. xxiii., p. 6.

specimens being much more protected from their enemies by their greater resemblances to objects rested upon, have by "natural selection" gradually assumed their present state. Note also, the wood has been much *collected* of late years, and the light specimens naturally are taken in much greater numbers than dark ones." This really is my own view, except that I think the darkening of the trunks arises perhaps less from the shutting out of light, than to the natural increase of humidity due to increase of age of trees and greater condensation, and to the gradual increase of Derby as a manufacturing centre.

Another extreme case, almost on all fours with this, is a most extraordinary development of extreme melanism in *Boarmia repandata*, near Huddersfield (*Ent. Mo. Mag.*, vol. xxv., p. 161). Mr. G. Porritt, F.L.S., in 1888, bred from a wood in that district some black forms of this species. So intensely black are these extreme specimens, that insects commonly called black, appear brownish beside them. The larvæ from which these specimens were bred came from an intensely dark fir wood, in the midst of what is one of the largest manufacturing districts in Britain, and where the vapour must hold in suspension large quantities of carbon and other impurities. An almost parallel case was cited by Mr. N. Cooke (*Entom.* x., p. 94) in a discussion of this subject, where he writes:—"The most interesting case of melanism that has come under my observation—and my friend Mr. Greening, of Warrington, can say if I exaggerate the facts—is the total change in the colour of *Tephrosia biundularia*, in Delamere Forest. Some thirty years since, when he and I visited Petty Pool Wood, this species was very abundant, appearing in March, and was to be found through April and May, but all were of a creamy white ground colour; dark varieties were so scarce that they were considered a great prize. Now it is the reverse, all are dark, smoky brown, approaching black; a light variety is very rare. The same change, and nearly to the same extent as regards numbers, has come over *Amphidasys betularia*. Throughout the district, from Petty Pool, including Warrington, to Manchester, the black form is now usually found. I am inclined to suspect that climate and manufactures have done more to bring about this change than anything else. During the past thirty years, what large towns have sprung up to the west of this district! Runcorn, Widnes, St. Helens, Earlstown, Wigan, etc; all pouring forth from their tall chimneys chemical fumes and coal smoke, which emanations are carried over our collecting

grounds by every westerly wind; and the wind is westerly[1] for nine months out of the twelve. This may have effected a change in the climate (*Entom.* x., p. 94). And yet, after coming to this reasonable and satisfactory conclusion, Mr. Cooke would not allow that "natural selection" had any hand in the matter, because he could not see the difference between "sexual selection" and "natural selection." Had Mr. Cooke seen the line of argument taken by Dr. F. B. White (*Entom.* x., p. 126), I think he must have been driven to the conclusion that I have (in common, I believe, with many others) arrived at. Dr. White (*Entom.* x., p. 127) says himself: "But what is the exciting cause of this tendency to variation? I think it is (in some cases, at least) meteorological, that is to say, cold or heat, dryness or dampness, presence or absence of sunshine, etc." So that even in these earlier discussions, atmospheric effects held a place, but generally a most subordinate one. In *Entom.* x., pp. 152, 153, Mr. Cooke also writes: "I have also lately become acquainted with the fact that *Tephrosia biundularia* of the dark form, occurs in the "black district" of Staffordshire—in Burnt Wood—not far from which an immense quantity of smoke is produced, by the manufacture of iron and pottery." Criticising Mr. Cooke's article (*Entom.* x., pp. 92–96) Dr. White says: "This aberration *Delamerensis* var. of *biundularia*, and the ab. *Doubledayaria*, Mill., of *Amphidasys betularia* are, however, exceptional cases, and cannot be considered as throwing much light on the origin of the majority of melanochroic forms" (*Entom.* x., p. 128).

Here I differ from Dr. White. They throw a great deal of light on the origin of the melanism in tree frequenting species. The colours of these species lend themselves more readily than those of most species to this peculiar form of variation. Being of a white ground colour with the principal markings black, it follows, that any darkening of surface will be more favourable to those specimens which have an excess of dark colour, than to those which have an excess of light. "Natural selection" will complete the work, and "hereditary tendency." will ensure its intensification. Again, the same species has undergone the same change at Derby (*Ent. Mo. Mag.*, vol. xxiii., p. 6), and has lately exhibited itself to some extent at Birmingham, and in the neighbourhood of Mansfield, Notts.

To show that these are not isolated cases of melanism of this particular class, and that the results obtained in *Boarmia*

[1] South-westerly would have been more correct.—J.W.T.

repandata, etc. are capable of generalisation, I may add that *B. rhomboidaria* var. *perfumaria*, *Diurnæa fagella*, *Hybernia marginaria* (*progemmaria*), and other tree frequenting species are excessively dark from the Huddersfield locality, and some of the specimens of *Cidaria russata* are blacker from this locality than from any other locality I know, except perhaps from the neighbouring one of Sheffield. *Arctia mendica* has also undergone a remarkable change near Huddersfield, *vide Trans. Ent. Soc.*, pp. 441, 442, and Pl. xiv. (1889).

I will cite one more example from our literature on this subject. It is written by Mr. Edelston, and refers to *Amphidasys betularia*. It is as follows:—"Some sixteen years ago, the "Negro" aberration of this common species was almost unknown. Last year I obtained the eggs of a female of the common form, which had been crossed with a "Negro" male; the larvæ I fed on willow, and had this year some remarkably pretty aberrations, the connecting link between the "Negro" and the usual form, but far before either as regards beauty. I placed some of the virgin females in my garden, in order to attract the males, and was not a little surprised to find that most of the visitors were the "Negro" aberration; if this goes on for a few years, the original type of *A. betularia* will be extinct in this locality" (*Entomologist*, vol. ii., p. 150, and *Ent. Mo. Mag.*, vol. xiii., p. 131).

I will now quote a statement of Mr. Cooke (*Entom.* x., p. 151), which appears to me to illustrate the effect of dampness due to woods on the development of dark forms of lepidoptera. Mr. Cooke says:—"Since my paper appeared, I have bred two dark varieties of a light-coloured species from the chalk. I never saw or heard of a dark specimen of the species before last year, and it puzzles me to account for them, as other dark specimens have been procured from the same *wood* on the chalk. Had it not been so, I should have concluded that the soot on my tree was the cause of aberration; but it is not so, for it is evident there is a dark race of this particular species occurring on the chalk a long distance from any manufacturing district." Just so, I can cite parallel cases in Kent, but there is no doubt the *wood* is the cause of it; the dark race is more readily protected in the wood, where the light race would be prominent, on the other hand, on the chalk itself the white race would be protected; hence, by "natural selection," the darker wood variety would be perpetuated in the wood, although the greater number of specimens, if it were

a generally exposed species, or one that usually rested on the ground, would be of a lighter race; *Gnophos obscurata*, *Eubolia bipunctata*, *Anaitis plagiata* offer almost parallel cases in Kent. I would here again call attention to the fact that Dr. F. Buchanan White (*Ent. Mo. Mag.*, vol. iv., p. 248) suggested the absence of wood from Breadalbane, as one of the reasons of the difference between insects from that locality and Rannoch:—"Breadalbane, however, has higher mountains and less wood, which may perhaps account for the difference."

In *Ent. Mo. Mag.* xxv., p. 40, Dr. Chapman writes:—"Melanism appears to be a western, rather than a northern form of variation; to be associated with a wet, rather than with a cold climate; and it has certainly been more common of recent years, which may be attributed to the long succession (unprecedented) of wet seasons we have recently passed through. My observation was on *D. fagella*. Twenty years ago this species afforded here an occasional dark or even black var. Happening to meet with one of these, I searched carefully for two seasons, but only got one black and two dark specimens. For the last year or two (result of wet seasons) they have been fairly numerous. Visiting certain oak trees with a lantern one night, lately, and the same observations might, occasion favouring, no doubt have been made during the day, I found the dark var. quite numerous, and about one to three of the ordinary form." Strange to say, the same thing has happened in this locality, the dark var. of this same species may now be frequently obtained in this district, and the general colour is undoubtedly getting darker; in fact, last March, I obtained a specimen in Shooter's Hill Woods, near here, as black as the Huddersfield specimens. Many other species, *Cuspidia* (*Acronycta*) *psi* for example, are darker in London than a few miles out, and I have little doubt that "natural selection" does this work, considering the general darkening of surface, which objects in London undergo. But the smoke of London has less intensifying force, so to speak, than that of Yorkshire, because the atmosphere is less humid, and hence the melanism of lepidoptera in this district is less general and intense than in localities, where, with an equal quantity of smoke, there is a greater rainfall or more humid atmosphere.

I have heard many observant and thoughtful lepidopterists attempt to correlate dark forms occurring in woods, etc., such as I have previously referred to, with the deficiency of light in such situations, and attempt to argue that this deficiency acts,

in some unknown way, on the larvæ which live in these dark situations, in the production of these forms. I cannot understand myself, how light can affect larvæ in this way, and I believe, there is nothing in the absence of light, which can lead us to assume that it is a primary influencing cause in the production of these forms. Again, I have heard the same reason advanced as an explanation of such dimorphic forms as those of *Hypsipetes elutata*, where a generally larger and more brightly coloured form feeds on willow and sallow, and a smaller, and generally darker, form occurs on bilberry. The two forms often occur side by side in the same districts. The difference in size, I believe to be entirely phytophagic, but the difference in colour,[1] I assume to be due to the same causes as in other melanic forms, viz., the darker surroundings which protect darker forms, and thus by "natural selection" give us a darker race. The large *elutata* of the south rest among the leaves and on the willow stems, the moorland forms chiefly on the ground. Mr. Birchall's opinion, before referred to (*Record*, p. 55), would be much of this character, and throughout his paper (*Ent. Mo. Mag.*, vol. xiii., p. 131) we find that he considered the absence or deficiency of sunlight, by some occult action on the larvæ, to be the cause of melanism.

"*Natura non facit saltum*" is a well-known saying among naturalists. Changes are not brought about in nature suddenly, and in no natural study is the saying more true than in our own interesting pursuit. There is no doubt that the tendency to melanism is brought about gradually, when once commenced, by "natural selection," and increased by "hereditary influence." But I was unprepared for the startling (to me) suddenness with which many species, during the wet season of 1888 assumed a darker colour. In none of the lepidoptera that I met with, was this perhaps more strikingly illustrated, than in *Lithosia pygmæola*, *Xylophasia monoglypha* (*polyodon*), *Agrotis corticea*, and *Agrotis tritici*, although others showed this influence. *L. pygmæola*, which is in favourable weather so exceedingly abundant on the sandhills of Deal, is, as is well known, usually of a pale yellow colour in the males, and of a darker shade or grey in the females; occasionally, however, a darker specimen than usual occurs, showing that there is a tendency in the species to vary in this direction; but in the summer of 1888 a very large proportion of the speci-

[1] In some districts, the large form is quite as dark as the smaller, where such coloration is of advantage to the species.

mens were exceedingly dark, and I have in my cabinet some strikingly dark specimens, although the species was scarcer than in any year since I have collected at Deal. *Xylophasia monoglypha*, which is usually of the pale variegated form, with an occasional var. *obscura*, were that year in greater abundance than usual, but almost all were var. *obscura*, some approaching var. *brunnea*, and one striking specimen of var. *infuscata*. In another very wet season (1872), a black *polyodon* was recorded from Sheerness. Again in 1888, at Deal, it was difficult to find anything but dark *Agr. corticea*. In some seasons, specimens as pale as ordinary ♂ *A. cinerea* are not rare, with a fair number of dark and black vars. In 1888 almost all were dark and black, scarcely a pale var. was to be obtained. Beautiful whitish vars. of *tritici* are not rare in ordinary seasons; that year they were comparatively rare, and the series I brought home was darker than any I have previously captured. There is no doubt that a succession of seasons like 1888 would intensify these varieties, to the almost utter exclusion of the paler forms. I think this illustrates the fact, that meteorological influence has played a great part in the sudden darkening of certain species, and that, given a number of seasons suitable to this particular development, the dark would altogether outnumber the pale forms.

I am not alone in noticing the influence which a wet season has on the sudden development of variation in certain species. In the *Entomologist*, vol. xxii., pp. 38, 39, Mr. W. W. Smith, of Ashburton, New Zealand, writing of the variation of *Argyrophinga antipodum*, says:—"There can be no question that many species of lepidoptera are greatly influenced in all stages by changes of temperature or by the seasons; and from notes on the variation of this butterfly in relation to the seasons, made during many years, I am in a position to show that the species exhibits greater variation in some years than others." Mr. Smith then goes on to show that in the dry seasons, 1881-83, there was little variation. He then writes:—"Then followed the wettest winter, and equally wet and coldest summer on record in New Zealand; every day of bright sunshine produced the emergence of some specimens of *A. antipodum*, and fresh individuals continued to appear until much later in the season than usual. In the same year (the early months of 1884), I collected the most variable forms of both sexes I had hitherto obtained." The years 1885, 1886, and 1887 were drier, and produced less variable specimens, but "last summer (1888), which was preceded by a very wet

winter, there was again a greater number of more richly marked and variable forms. From the foregoing notes it will be seen that the variation of the species is most predominant in seasons succeeding wet winters. The same remark applies to *Chrysophanus boldenarum*. How the seasons operate in producing this effect requires careful investigation," etc. Mr. Smith then adds:—"An abundance, scarcity, or change of food, and its environment during the larval stage, are known to produce great variations among certain species; with *A. antipodum*, the finest marked and best developed forms are evolved in humid seasons, and such succeeding wet winters. . . . . I am not in a position to say that the tendency to darkening in the colours of the species is, in humid seasons, a case of atavism; but I incline to such an opinion, as the preceding geological period in New Zealand was immensely more humid[1] than the present, a fact which I think favours such a view."

Here, then, is a positive proof of a primary exciting cause, and, if nature develops dark varieties in wet seasons more than in others, and, in wet seasons, species normally invariable, are more or less variable—*vide Entom.*, vol. xxii., pp. 37–39, as well as my own remarks on the sudden development of a melanic tendency in wet seasons—I think it a fair deduction to assume that the variation is primarily the result of meteorological influence, and that humidity is essentially the exciting cause.

There is no doubt that "natural selection" is almost entirely the direct cause of the variation in colour (both melanochroic and leucochroic) of species which rest upon the bare ground, and that the insects which assimilate most completely to the geological strata on which they rest, are those which are preserved in the struggle for existence, and which finally establish a particular form in the district. NOCTUÆ and many GEOMETRÆ rest on trees, fences, or the ground. Many rocks, and almost all trees and exposed fences are darkened by rain,[2] hence the assimilation becomes more complete in naturally wet and humid districts. But to take a special case where the direct influence of humidity can, compared with "natural

[1] It is more than probable that this was so in Britain, as the country was, in comparatively recent times, much more covered with forests and woods than at present, and this alone would make the country more humid.—J.W.T.

[2] Dr. Chapman writes:—"In your paper (*Ent. Record*, p. 56) you criticise the idea of rain *permanently* darkening rocks, trees, etc., by pointing out that they dry in the intervals, but that is precisely what they do not do. Such objects are often wet, especially in some localities, for weeks together, even in the summer; of course they do dry sometimes, but generally they retain one lot of moisture, until they get another, during the greater part of the summer" (*in litt.*)—J.W.T.

selection," have very little to do with the variation in colour. *Gnophos obscurata*, as is well-known, is one of the most striking examples. I devote a drawer to the series of this species in my collection, and what do I find? I find that among the specimens captured on the chalk a large percentage of pale forms occur, assimilating most perfectly to the chalk on which they rest; but even here, in the more exposed situations, there is general variation within narrow limits in both sexes, and the females are generally darker than the males. I have always supposed this latter phase to arise from the fact that the females appear to rest more among the herbage than do the males. On very impure limestones grey forms occur, assimilating most perfectly with the limestone; while on peat, dark peaty-black coloured specimens are found. On slate formations intensely dark slate-coloured or black specimens are the ordinary form, while I have very red specimens from the red sandstone formation. Here is a complete response to environment, and I leave my readers to infer how special races of this particular species have been brought about, and why, on rocks, naturally dark or artificially darkened by rain, black forms are most generally found. The same line of variation is traceable in *Boarmia repandata* in districts where it settles and rests on the ground, and not on the trunks of trees, as is usual with us in the woods of Kent and other southern counties. I have specimens varying in colour exactly in the same way as those of *G. obscurata* just mentioned, viz., from whitish to black. In these species both the pale and dark races are those which have been developed by "natural selection" in the particular localities in which they are found. Although humidity or wetness, by darkening the surfaces on which the insects rest, would intensify the colour, yet the particular geological formation is here the prevailing factor, or why should we get white forms on the purer cretaceous strata, grey on the impure limestones, red on the red sandstones, black on the slates and schists, and brownish-black on peat? In short, why is there such a complete response to environment? Speaking of these leucochroic and melanochroic forms, Dr. Buchanan White, *Ent. Mo. Mag.*, vol. xiii., p. 149, makes the following remarks, which my own observations have since materially borne out. He writes:—"That there is exciting cause for both forms I am persuaded, because we find that species, which are sufficiently common for us to observe year after year in abundance, are found to be much more subject to variation in

some years than others; and if (presumably) the meteorological differences of one year from another, cause, in a single locality, a varying amount of variation in species, we may reasonably conclude that the meteorological differences between one locality and another, continued year after year, will tend to variation in different directions in the individuals of a species common to both. Then, if in one locality, where the struggle for existence is greater, a peculiar variation is found to carry advantages with it, "natural selection" steps in and does the work, and, if the advantage is very great, may eventually result in that particular variety supplanting all others. If the advantages were not so great, the particular variety would not be so particularly favoured; and if there were no special advantage (but still no disadvantage), then the variety would only be on an equal footing with the other forms, and individuals of that character would vary in number from year to year, according as the meteorological conditions (presumably the exciting cause) varied, with a certain percentage for heredity."

This is in general terms what my own observations have suggested (*Entom. Record*, vol. i., pp. 121, 122), and is borne out exactly in Mr. Smith's paper, "On the variation of *Argyrophinga antipodum*" (*Entom.* xxii., pp. 38, 39) previously referred to, where he shows that this species varies in proportion to the general humidity of the season. No better example of the incidental reasoning, in the extract quoted above, could be given than the BRYOPHILIIDÆ—*muralis* and *perla*. The most elementary student of practical lepidopterology will be able to apply the doctor's reasoning to these species, and I will not weary my readers with further explanations of such simple phenomena. Dr. Chapman also suggests:—"There is no doubt that the geology of a district would have some importance—first, as to the colour of the rocks and soil, and secondly as to the colour of the lichens that were most abundant" (*in litt.*).

I have heard it suggested against this theory, that the insects from marshes and similar situations are pale coloured, and, as instances, the LEUCANIDÆ have been given, on the assumption, I presume, that all insects from such localities should be dark, but this does not appear to me to militate in any way against the theory. I, fortunately, have been able to study the actual habits of some of the species of this family in a state of nature, and, as they sit head downwards, with their wings folded closely to the edges of the reed on which they usually rest, the appearance they have so closely resembles the nodes

in a reed culm, that it is hardly possible for a trained eye to detect them, and we may be certain that the peculiar colour, which is so advantageous in enabling them to escape their enemies and thus perpetuate their species, is as directly dependent on "natural selection," as is the darker assimilating colour of those species which rest on the ground, fences, or other dark objects. As well say, because some particular genera of marsh-frequenting moths are pale, that all should be pale, as, that because some Rannoch species are white, all should be white. An argument of this kind would be ridiculous until we know the particular habits of each species. But with regard to the pale, marsh-frequenting genera, I have specimens which prove distinctly that even these species vary according to general humidity coupled with an area in which large quantities of smoke are being produced, *e.g.*, *Leucania impura* and *L. pallens* produce much darker specimens in the London marshes than in the open fields near Strood, in Kent, and Scotch specimens are occasionally darker still. Again, *Agrotis nigricans* is blacker from the Greenwich marshes than from the fields and marshes in the neighbourhood of Rochester, where they are generally much marbled with yellow, and the same (Greenwich) marshes give a much larger percentage of dark specimens than any locality I know of a drier character. *Apamea fibrosa* has a larger percentage of pale variegated forms in its more open localities. Many examples of this kind could easily be adduced in the same direction. But it must be remembered that the Fen District of England is probably the *driest* part of the British Isles, so far as actual rainfall is concerned, and this must be carefully borne in mind when considering the species obtained in this district, or those of a similar environment.

Another query based on the same line of argument is occasionally heard. "Why do we get white butterflies and black moths on the same geological strata?" Such a question always appears to me to show entire ignorance of the subject of "natural selection." The colours of butterflies assimilate generally to the flowers or other objects on which they rest. Their wings are drawn up over their bodies when at rest, and we only see the undersides. It is, therefore, to the undersides we must especially look for variation, and here we chiefly find it. The variation of the undersides of our most common butterflies is a most interesting study, and opens up a wide field to those who have never looked for variation, where, in this group, variation must show itself. Of course the variation

of butterflies on the underside is often reproduced in a mild form on the upper side, but, from a scientific point of view, the variation of the upper sides is practically valueless, compared with the variation of their undersides. Incidental cases of variation occasionally occur on the upper side of butterflies, but as a general rule such varieties are, in my opinion, constitutional, and each must be considered on its merits as to its probable mode of causation.

These undersides, serving as they do for a protection to the insect, must be studied then, if we are to find the changes which "natural selection" plays in this group. As an example of the influence of environment on a butterfly, I will instance *Pieris napi*. In our southern districts of England it is generally very clear (although showing seasonal variation within certain limits), white on the upper, and yellow or greenish grey on the under surface. In many Scotch and Irish localities it is normally much darker and more suffused, and on the Alps the spring form—var. *bryoniæ*—is very dark. But in the South of England, *Pieris napi* settles on leaves, where its yellow or greenish underside protects it to a great extent; while in the Alps, the extreme form frequently settles on the bare ground, or on vegetation more or less blackened by cold and exposure, where its darker coloration is now particularly protective. The undersides of *Vanessidæ* bear great resemblance to dead leaves, the yellow and greenish undersides of *Pieridæ* to green leaves; the speckled undersides of *Lycenidæ* to the inflorescence of grasses, and, indeed, it could easily be proved that every particular species has its undersides particularly developed to suit its surroundings, *e.g.*, *Euchlöe cardamines*, *Hipparchia semele*, and *Thecla rubi*, are striking examples. I think I have shown that all our butterflies (for purposes of variation) should be studied from their undersides, and have cited enough examples to show that white or other coloured butterflies will continue to exist side by side with dark moths, and that such facts are not at variance with the theory advanced. Geometers which rest in the same position as butterflies will also have to be studied from the same standpoint. The genus *Selenia* occurs to me at the present moment.

Again, a thoughtful lepidopterist says:—"If your theory be true, then the same effect ought to produce the same result. Take for example *Polia chi!* Why in Durham are the dark specimens of *chi* always inclined to var. *olivacea*, while the dark var. in Yorkshire is so different?" The reason is not far to

seek, but our lepidopterists must bear in mind that we have two[1] strong causes at work:—(1). The exciting cause in the production of melanism, which I assume is moisture; (2). The general protective influence of "natural selection." The same cause (moisture) does produce the same effect, viz., the darkening of the ground colour, but the second influence of environment, governed by "natural selection," plays a still stronger part, and I would again ask my readers to bear in mind that, if anything in the environment tends to the protection of a certain form, that form will be produced to the almost entire exclusion of all others, and a permanent local form will be established. In *Polia chi* these forces are undoubtedly at work, and, until my readers will master the fact that any secondary influence may modify, or almost entirely obliterate the original effect, very little can be done towards the mastery of this subject.

It has been suggested, and I have known it assumed as fact, that exposure to cold frequently darkens the colour of certain double-brooded species of lepidoptera, and have seen *Selenia illustraria* brought forward to illustrate the assumption. But, if cold could aid, in any way, the production of melanic forms, why, in Britain, do we get our most melanic forms on the west coast of Ireland, where, owing to the influence of the Gulf Stream, the temperature is, for the latitude, exceptionally high? It is also a matter of fact, that species from dry open areas in the high latitudes of America and Europe present no traces of melanism.

The first writer on the subject of dark coloration being produced by a low temperature, was Professor Weismann. To me, his experiments, so far as lepidoptera are concerned, appear of little or no value. The incidental influences of moisture, etc., appear to have been overlooked entirely, and his experiments were too general to be of much service. Mr.

[1] I would ask all my readers who pay attention to this subject, not to form hasty opinions where their observations do not appear to coincide with my conclusions, but to take into account all the surrounding circumstances, and above all to have plenty of material for comparison. It is useless to form an opinion on the difference between a few (say less than a score or so) specimens from different localities, and equally useless to compare specimens from comparatively adjacent districts, *e.g.*, Lancashire and North Wales, and expect a striking difference. Often there will be, but more often there will not. *Aplecta nebulosa* from Glasgow is occasionally of a beautiful white colour, altogether different from our South of England specimens. Following out my theory the Glasgow specimens should be darker, but some outside force undoubtedly modifies the species in this district, with the result that it differs in the way I have pointed out. I do not doubt that if I saw this insect *in naturâ* at Glasgow, I could easily explain this apparent opposition to my theory, but I know nothing about its environment there.—J. W. T.

Merrifield has been paying considerable attention to the influence of temperature on seasonably dimorphic species, and his general conclusions (*Transactions of the Ent. Soc.*, 1889, pp. 78–97) are of much greater value. Professor Weismann principally directed his attention to the application of a low temperature to the pupa, and considered, that retardation in this stage, was the principal factor in producing a dark coloration in a winter form of a double-brooded species, where the seasonal forms were dimorphic in colour; but I believe myself, that the influence (whatever it may be) which produces a dark coloration in these species, acts but very little (if at all) in that stage, and that the larval stage is the one most affected by the exciting influence. If this darkening takes place by retardation in the pupal stage, then, the insects of northern Europe and America, instead of being, as they are, more than usually pallid, would be excessively dark, for many north-country species appear normally to pass two years in this stage. It is in mountainous and other humid districts, that we find dark-coloured insects developed, and, since the spring specimens of several seasonally dimorphic species (*Tephrosia crepuscularia*, *Selenia illustraria*, etc.) are darker coloured than the summer specimens, I think it more than likely, that delay and environment have infinitely more influence in the larval stage, than the same influences can possibly have on the comparatively quiescent pupal form. It is, therefore, advisable to note, with regard to these dimorphic seasonal forms, that Mr. Merrifield writes:—"In the double-brooded species, the governing consideration is, which brood am I to belong to—the summer form or the winter form? and that decision can only be arrived at in the larval (growing) stage, and may be then controlled by external influences, *e.g.*, temperature.[1] I do not believe that temperature[2] can ever convert the one form (summer or winter) into the other, unless brought to bear on the larval stage" (*in litt.*). Again, after all the care that Mr. Merrifield has taken with his "temperature" experiments, he writes:—"Like you, I am by no means prepared to accept the position that cold causes melanism. *I think it quite probable that, in some cases, it may produce an opposite effect*" (*in litt.*).

I will now quote one or two more of Mr. Merrifield's observations, which appear to me of importance, and the value of

[1] If accompanied by humidity.

[2] Mr. Merrifield's experiments were based on temperature. He therefore assumed that all his results were produced by temperature.

these remarks is enormously increased, when we consider, that, having his attention directed especially to "cold" as the cause of the darkening, he could not reconcile his results as being borne out by differences of temperature. He writes:—"So far as my experiments on pupæ have gone, the results of them appear in close accordance with those of Professor Weismann, but other experiments seem to indicate that the temperature[1] to which the larva is exposed in its growing stage has much to do with the colouring of the perfect insect" (p. 82). Mr. Merrifield also writes:—"As to the causes of variation in *colour*, markings and shape, the inquiry is a more complicated one, of course embracing the pupal stage, though I am inclined to think, for reasons I will give, that it should by no means be confined to that stage" (p. 84). The first observation relates to *Ennomos autumnaria*, which were bred in the "wet" and cold summer of 1888. There were three sets: one kept out of doors and exposed to wet and cold; a second kept indoors in a "drier" and warmer atmosphere; whilst a third was kept indoors but "bottled." Mr. Merrifield, treating his as "temperature" experiments, supposed the differences in these sets to be due to the difference in temperature. I consider the difference due to the difference of the "wet" and moist conditions to which the larvæ were subjected. Mr. Merrifield writes:—"In *autumnaria*, the difference (between these first two sets) is most marked and very conspicuous. . . . . Generally, if not always, the spots and marks of the forced ones are less dusky, *and not nearly so dark;* nearly all the males, and all but one of the females, have fewer spots than the corresponding sexes in the sleeved; on the underside[2] the differences are more strongly marked. The general result is, that of the 26 forced, there is only one that comes up to the general standard of the 24 sleeved in abundance and in darkness of spotting and other marks, and there are only 3 of the sleeved, which, in lightness of spots and marks, approach the general hue and appearance of the 26 forced" (p. 86). That is, those that were exposed to the wet season of 1888 as larvæ, were much darker than those bred indoors and protected from these conditions. Mr. Merrifield, regarding this experiment, goes on to say:—"The difference in appearance between the forced and the sleeved being so marked, I give in detail the

[1] I consider these differences caused by the more "wet" condition.

[2] This tends to prove my previous statement, as to looking for variation on the underside of butterflies. This moth rests like a butterfly with its wings drawn up over its body.

facts, which appear to indicate that, in this case, the conditions to which the larvæ were subjected may have had much to do with the very striking difference in the moths. One of two conclusions, at all events, seems almost to follow from the experiments, viz., that the larval period was the critical one, or that the colour of the perfect insect, in this single-brooded and summer-pupating species, can be affected by exposing the pupæ to a very moderate difference of temperature." Mr. Merrifield then goes into particulars of these two broods, together with the third or "bottled" brood. But, in all these particulars Mr. Merrifield argues as if only temperature were to be noticed, although he states that whilst there was so much difference between the "forced" and "sleeved" imagines, the "bottled," kept indoors at a comparatively high temperature, and which, if temperature were the prevailing factor, ought to be more like the "forced," turned out to be, "in colour and intensity of marking, not far from the sleeved." The process of "bottling" at once tends to place the larvæ under moist and humid conditions, which seem to have affected these in the same way as the rain of 1888 affected the sleeved. Mr. Merrifield then writes:—"If the great difference between the forced and the sleeved originated in the pupal period, it would seem to follow, that the difference between about 66° and about 75°–80°, during an exposure of a very few weeks, is sufficient, in this species for the purpose" (p. 89). Now, I would ask, how, in the name of common-sense could this be? If this were so, pupæ subjected to this difference of temperature would produce two forms of the same moth, and every *autumnaria* spending its pupal period at 66° should have a tendency to melanism. Other species, I presume, should follow the same rule. Nothing, I think, better shows that Mr. Merrifield was right, when he wrote:—"I think the *larval* period is the critical one," and again:—"I do not think temperature can ever convert one form into the other, unless brought to bear on the larval stage," than the extract just quoted. There is no doubt the action of moisture (it may be more or less affected by temperature at the same time, but I fail to see how) on the larva is the cause of the darkening.

Professor Weismann had recourse to a very far-fetched notion to explain the reason why he supposed, that, although a difference of 14.9° R. (33.5° F.) between the German winter and summer temperatures failed to originate a climatic variety of *Papilio podalirius*, a difference of 9° F. between the

summer temperatures of Germany and Sicily was sufficient to do so; his suggestion being, that the cause was to be found in the absolute temperature reached; and I am rather surprised that Mr. Merrifield seems inclined to follow him in (what appears to me) his fallacious reasoning. However, for our purpose, it is sufficient to consider that Professor Weismann and Mr. Merrifield are both obliged to leave the "cold" theory, to explain in other ways, why climatic races are developed in countries with high temperatures. In short, the "cold" theory of melanism can only be maintained or bolstered up by finding different explanations for almost every different case. I have no doubt, that, if Mr. Merrifield's attention had been directed to humidity as a cause of darkening, my own ideas of melanism would have received substantial corroboration. I think no better proof of the small effect "cold" can have in darkening colour, could be produced than Mr. Merrifield's remarks on "icing." He writes of some moths, produced from pupæ iced for a considerable time :—"The cooling in this case did not produce any change of colour, or, if any, it is a very slight one." Again, about another experiment, Mr. Merrifield (p. 93) writes:—"The later ones of the 8 are very much darker than the earlier ones, and, though the progressive darkening is not quite regular, it is difficult to look at them without thinking that retardation of development has been the operative cause, *the retardation in this case not being associated with cold*, as the pupæ have rarely or never been at a lower temperature than 60°. There are some other facts which rather favour the view that retardation, *without cold*, may be a cause of, or at least be associated with, darkening," etc.; and again (p. 94), "there is no marked difference (in colour) between those which have been frosted and the rest." What a pity it seems that Mr. Merrifield's attention had not been directed to the incidental circumstance of "humidity," as some most valuable results would undoubtedly have been obtained.

Lord Walsingham's paper on Melanism, which has attracted some attention, is also based on temperature. His paper can hardly be said to propound a theory, as it scarcely seeks to show what is the cause of melanism, but that, melanism once having been produced, the dark coloration is an advantage to the insect possessing it; it then goes on to suggest that this advantage, therefore, is the cause of the insect being melanic; although, how it can in any way be looked upon as a "cause" of melanism, I fail entirely to see. But we have to bear in

mind that, although the whole of Lord Walsingham's theory (?) (*Presidential Address to the Yorkshire Naturalists' Society*, 1885) that melanism is probably due to the rapidity with which the insects would absorb heat because of their darker coloration, is extremely interesting and full of good reasoning, yet it is entirely misleading, because based on entirely fallacious data. The assumption, that insects from high latitudes are generally melanic, is entirely erroneous. They appear to be in some instances, if we confine ourselves to the British Isles, or even to the western shores of Europe under the influence of the Gulf Stream, but, in the high latitudes both of Europe, Asia, and America, especially at low altitudes, melanism is practically unknown. Mr. Dobrée (*Entom.* xx. pp. 25–28) thoroughly exposed this fallacy, and showed how incorrect it was, and Mr. Jenner Weir (*Entom.* xx. p. 85) supported Mr. Dobrée's facts; and, since the whole of Lord Walsingham's theory was based on this incorrect assumption, the misleading character of the theory may be easily estimated. The actual fact "that dark colours would be advantageous to insects, owing to the rapidity with which they would absorb heat," is a simple statement of fact that the most elementary student of physics would understand; but that this is the cause of melanism is another matter, especially when it is assumed, that, as the temperature gets lower, the moths get darker, which is contrary to fact, unless the lower temperature is accompanied by increased humidity. Lord Walsingham, too, allows nothing for the fact that moths generally fly by night, and that radiation would have to be taken into account, as it is another elementary fact in physics that "good absorbers are generally good radiators;" but still the previous fact, that the moths of northern latitudes are *not* generally given to melanic tendencies, renders most of the arguments in the paper equally abortive.

Mr. Dale, in the *Young Naturalist* Supplement, Jan. 1890, pp. 37, 38, after pointing out that melanism is essentially characteristic of the most humid districts, asserts that:—"The dry chalk soils of the South absorb a greater amount of heat than the wet peaty soils of the North, and to that, in conjunction with the fact that there is more rain and consequently less sunshine in the North, is melanism entirely due." Grandly assertive, but entirely unconvincing, this was rather severely handled by Lord Walsingham in the February number of the same Magazine, but Mr. Dale again comes to the front, and

begins by denying one of the most elementary laws of physics, viz. :—that dark-coloured substances do absorb more rapidly than light-coloured, and then adds :—"The pith of my argument is this—dark-coloured soils absorb a great deal of moisture and cannot absorb so much heat as light-coloured soils, which are dry." Here Mr. Dale assumes, evidently, that all dark soils are necessarily "heavy" (*i.e.*, retentive) soils, and *vice versâ*, an obvious error. The amount of heat absorbed by the soil depends on the quantity of water in it *which does not drain through*—in other words, on the quantity of evaporation to be effected by the sun's rays, before they can commence warming the soil. Side by side and equally well drained, a dark clay absorbs more heat than white chalk. Mr. Dale's statement above—"Consequently less sunshine," etc., is again difficult to understand. More rain does not always mean less sunshine. Probably the most brilliant sunshine in the world is found in India, and yet some parts of India register above 400 inches annual rainfall. In fact, Mr. Dale's notes are scarcely worth serious criticism ; they offend against the most elementary facts of geology and the most elementary laws of physics.

I think cold may therefore be looked upon as quite a minor factor in the production of melanic forms, even if it may not be altogether neglected, for it is clear that the coldest areas do not normally produce dark-coloured insects, and if cold areas do not produce melanic forms in nature, I think it a satisfactory deduction that experiments, based on temperature, which appear to give results tending to show that a low temperature does produce dark-coloured forms, should be looked upon with grave suspicion unless all attendant circumstances have been fully taken into account. At present, I am inclined to look beyond temperature for the explanation of results, which appear to have been produced in experiments based on temperature, and there is no doubt that, had the experiments been carried out with a view of testing the influence of moisture, much more satisfactory results would have been obtained.

Before leaving altogether the general question of an exciting cause, there are one or two points which we should not overlook in our inferences and experiments. The first is the influence of "heredity." Throughout my paper I have assumed that my readers would always take this comparatively and undoubtedly inconstant (in different species) factor into ac-

count. But, it appears advisable to mention the fact here, because, in certain experiments, its influence might be so potent, that it would be really the most powerful factor acting at the time, and yet its influence might be altogether overlooked. Under the artificial conditions of domestication, we know this power is a very great and important one, and there is no doubt, that what is applicable to the artificial conditions of life in the higher animals, must influence, more or less, insects kept under similar artificial conditions. Where insects have a normal tendency to vary, and where artificial breeding gives all sorts of strange varieties under apparently the same or similar conditions, it is manifestly impossible to conduct exact experiments with a species having the influence of heredity strongly developed, with any probability of arriving at a satisfactory result. I have, for example, in my collection, specimens of *Selenia illustraria*, which might belong to either the spring or summer brood, so far as colour is concerned, and which would puzzle me entirely if I did not know to which brood they really did belong. Captured specimens of this species not only give variations in colour (in the same brood), but slight variations in markings as well; in our breeding-cages the variation is more pronounced. It is manifest here, that in this species "heredity" would be a naturally strong factor, and, if we add to the natural hereditary tendency to vary, the increased hereditary influences produced by an artificial condition of life, and the result of possible interbreeding, we see how potent a factor it might become. But this is not all. This particular species (and there are many others similarly inclined) has not only a natural tendency for the specimens of each brood to vary *inter se*, but it exhibits strong dimorphic coloration in its broods according to season, so much so, that we talk of the dark spring brood and the paler summer one; and, although in nature, these colours are generally constant, and even moderately so in our breeding-cages, yet, added to the previous hereditary influences I have shown to be at work, we see how strongly developed the sum total of heredity is, and how useless for any purpose of exact experiment relating to colour or markings, this species becomes. I have dealt thus fully with this species because it not only exhibits the hereditary tendency to a very full extent, but it is unfortunately (probably because it is so hardy and easy to rear) the species which has been generally selected for temperature experi-

ments.[1] The other species, *Ennomos autumnaria*, which has been used for experiment by Mr. Merrifield, is one which is much better suited for experiment, as its natural tendency to vary is less; but being excessively rare in Britain, the species is being interbred in vast numbers for our cabinets, and, in a few years, will be quite domesticated, and probably become absolutely useless for experimental purposes, as it is now (1889), more or less, from my point of view, for it is impossible to get new British stock except at very rare intervals. I think I have now written enough to show that experiments made under artificial conditions, are open to grave objection, and that we may not always assume that results are produced by the same forces in nature, as those by which similar results appear to be produced under our artificial methods of breeding.

The foregoing paragraph on heredity was written in the early part of the year 1889, and my assumptions now (Jan., 1891) appear to be receiving most positive confirmation. In the Report of the Meeting of the London Entomological Society, Dec. 3rd, 1890 (*vide Ent. Record*, vol. i., pp. 267 and 268), with regard to some further temperature experiments conducted by Mr. Merrifield, we read:—"Mr. Merrifield read a paper 'On the conspicuous changes in the markings and colouring of Lepidoptera, caused by subjecting the pupæ to different temperature conditions,' in which it was stated that the results of many experiments made on *Selenia illustraria* and *Ennomos autumnaria*, tended to prove that both the markings and colouring of the moth were materially affected by the temperature to which the pupa was exposed; the markings, by long continued exposure before the last active changes, but before the colouring of the perfect insect began to be visible; a moderately low temperature during this period causing darkness, a high one producing the opposite effect, and two or three days at the right time appearing in some cases sufficient. Dryness or moisture applied during the whole pupal period had little or no effect on either colouring or markings. Applying the facts thus ascertained, Mr. Merrifield said he had obtained from summer pupæ of *illustraria*, some moths with summer colouring and spring markings, some with spring colouring and spring markings, and some with summer markings but an approach to spring colouring. The specimens were exhibited. Mr. C. Fenn, who said he did not agree with Mr. Merrifield's conclusions, exhibited a very long and varied

[1] It was unfortunately one of those selected by Mr. Merrifield.

series of *Ennomos autumnaria*, all of which, he stated, had been bred at the same temperature. He expressed an opinion that the presence or absence of moisture, rather than differences of temperature, was one of the principal causes of variation." The members present appear to have been of opinion that Mr. Fenn's specimens, bred at the same temperature, exhibited a very much greater range of variation than did those of Mr. Merrifield, which were supposed to vary because of the differences of temperature to which the pupæ had been subjected. It appears abundantly clear that Mr. Merrifield's specimens would probably have varied (as Mr. Fenn's did) just as much under ordinary, as they did under special conditions, and that the variation in his specimens was probably due, as the variation in Mr. Fenn's specimens was apparently due, not to temperature, but to heredity, and other incidental causes combined.

The inference suggested by Mr. Merrifield, or, more correctly, the conclusion at which he arrived, appears directly opposed to the inference adduced from his previous experiment on *Ennomos autumnaria* (*Entomologist's Record*, vol. i., p. 231), and I should suppose that the temperature had really very little to do with the actual result, and that one might get as many results, and draw as many separate inferences, as one chose to make different experiments. If Mr. Merrifield's last inference be correct, it should be capable of proof by any competent and careful entomologist, who ought to be able, by dividing a batch of pupæ into two separate lots, and by treating them in the way Mr. Merrifield suggests, to obtain two sets of moths of different coloration, by subjecting them, just before emergence, to a high and low temperature respectively. But this is impossible, as collectors know who breed lepidoptera in large numbers and force their pupæ; and I can only repeat, that I believe the larval stage is the vital one, and that experiments on pupæ can give but negative results. Last winter, in January, I forced at a very high temperature a large number of specimens of *Mamestra persicariæ* and *Macroglossa fuciformis;* these were exceptionally dark, but those I left in a cool greenhouse, and which emerged much later in the year at a lower temperature, were equally dark, and I can only infer that the temperature to which the pupæ were exposed, did not affect the coloration, but that whatever tended to make these two broods darker than usual, must have affected the larvæ before they entered the pupal stage; and yet this is in direct opposi-

tion to Mr. Merrifield's inference, that the subjection of pupæ to a high temperature just before emergence, produces a pale coloration, and I can only assume that the variation in Mr. Merrifield's experiment was produced independently of the temperature, and was the result of some previous condition of the larval stage, or a simple result of heredity, or some other incidental circumstance not yet taken into account, and, in the case of *Selenia illustraria*, which Mr. Merrifield has been interbreeding for several years for other experiments, the result obtained may be, in part, due to a weakened (diseased ?) condition of the larvæ, brought about by interbreeding; but this phase of the subject is dealt with in another paragraph.

In close connection with heredity is the development of dimorphic seasonal forms, when one or other of the forms has melanochroic tendencies, and the inquiry as to how these forms may possibly be produced. With regard to "seasonal forms" Mr. Cockerell writes:—"*Papilio ajax*, spring form *telamonides*, Feld., and summer form *marcellus*, Bd. *Pieris protodice*, spring form *vernalis*, Edw., is smaller and darker than the summer form. *P. oleracea*, summer form often larger, and wings thinner and purer white than spring form. *P. rapæ*, 'Further south (in the United States) the winter (spring) form tends to pure white on the upper surface' (G. H. French). *Colias eurytheme*, spring form *keewaydin*, Edw., smaller than summer form, duller yellow and with less roseate reflection, both sides more sprinkled with black scales, the orange patch on the fore wings not so marked, sometimes almost absent. *Phyciodes tharos*, seasonal forms *marcia*, Edw., and *morpheus*, Fb.; *P. phaon* also has seasonal forms. *Grapta interrogationis*, hybernating form *fabricii*, Edw., summer form *umbrosa*, Lintn.; 'There are about four broods in a season, and while those of the last or hybernating brood are the pale forms, the others are more or less mixed, as Mr. Edwards has shown' (G. H. French). *G. comma*, hybernating form *harrisii*, Edw., summer form *dryas*, Edw. *Lycæna pseudargiolus* vars. *lucia*, Kirby, and *marginata*, Edw., coming from pupæ which have hybernated, show coalescence of markings on the under side of the hind wings. *Selenia bilunaria* var. *juliaria*, Haw., and *S. lunaria* var. *delunaria*, Hb. In North America, owing to the great difference between the summer and winter climates, seasonal forms are often very marked. As a rule, the spring form, emerging from pupæ which have hybernated, is smaller and darker than the summer brood, a condition which has been

attributed to the effects of cold while in the pupa stage. But, strangely enough, a moderate amount of cold appears to have an opposite effect, for in our climate the spring *Pieridæ*, which are dark in America, are distinctly lighter than the summer forms, and the imported *P. rapæ* would seem to have retained that character somewhat in America. Also, as regards size, I believe that it is found that the races of large silk-moths, which are bred on the cooler slopes of the Himalayas, are invariably larger and finer than those of the same species from the heated plains of India; and this has been attributed to their longer sojourn in the pupal[1] state, whereby they have more time for development. The only theory I can advance to harmonise these various facts may seem to many a somewhat unwarranted one, and I shall look with interest for any comments on it. The darker and smaller forms may be supposed always to be those whose development has been quick, the metabolism great in proportion to the growth, while the larger and lighter forms have developed more slowly, with a longer period of growth, and less intense metabolism; in short, as Mr. P. Geddes would say, the first have developed katabolically, the last anabolically. When eggs or seeds are subjected to a low temperature, although life is not necessarily extinguished, growth and metabolism cease. The winter in North America is sufficiently cold to have a like effect upon hybernating pupæ,—they live, but do not grow.[2] The summer comes on with comparative suddenness, and the hot rays of the sun throw them into the most intense metabolism, so that the imago emerges with the wing-structures, so far from having developed slowly and in the cold, presenting every evidence of rapid change. In a climate like that of England, however, the winter is not cold enough to entirely arrest wing-growth, and hence the spring emergences present usually some evidence of slow change accompanied by gradual growth. It will probably be objected to the above theory, that summer in America is at least as hot as spring, so why do not the summer forms present at least as much evidence of quick change? To which I can only reply, that I am inclined to suppose that the vital structures and organs of generation develop at a much

[1] I should consider it impossible that a longer sojourn in the pupal state, could make them "larger and finer." One can understand that a longer sojourn in the larval state, with plenty of food, would act in this direction.

[2] Develop rather than "grow." I believe all size variations to be phytophagic, and as such, to be determined in the larval stage.

lower temperature than the wings, of which theory confirmation will be seen in such examples as the normally wingless, but otherwise perfect, *Choreius ineptus*, which is said often to develop wings in unusually hot seasons. I hold, therefore, that the North American species hybernating in the pupal state attain a nearly perfect development of their vital functions before the spring, although wing-growth has been in abeyance; but those pupating and emerging in the summer cannot assume the perfect state until the time necessary for the growth of the vital and reproductive organs has expired, so that the wings, developing all the while, have no period of sudden quick change like those of the spring brood. The seasons in Europe were probably at one time much more marked, and possibly some of the Mediterranean species arose as summer forms of northern species, or *vice versâ*: for instance, in the genus *Gonopteryx*. *Colias eurytheme*, of North America, has in the summer brood an orange patch on the fore wings similar to that of *Gonopteryx cleopatra*, but the spring emergence has the patch reduced, and sometimes almost absent, thus resembling *G. rhamni*. Now, supposing the climate were to become more uniformly cold in the north and warm in the south, these seasonal forms would be transformed into geographical races, and ultimately species. Such may have been the origin of *G. rhamni* and *cleopatra*" (*Entomologist*, vol. xxii., pp. 27–29).

I must say that I do not altogether follow Mr. Cockerell's reasoning here as to "growth" in the pupal stage. The foregoing paragraph certainly leaves me with the impression that Mr. Cockerell believes some change in the size of the imago may take place by growth, in the pupal stage. I believe myself that the size of the imago is entirely determined in the larval stage, and fixed as soon as the larva becomes a pupa; but that different degrees and rates of development may affect not only the condition of the membranous structure of the wing, but the scale structure as well, is very probable, and whilst not at all prepared to criticise Mr. Cockerell's statements as regards development and its effects, I do most certainly believe "growth," as determining size, impossible in that stage. I have before hinted that, possibly, delay in the pupal stage may to a small extent help to bring about such changes as occur in seasonally dimorphic species (although I believe all the greater changes result from delay, etc. in the larval stage); and since cold produces delay both in the larval and pupal stages, cold may thus indirectly influence the colour of insects

to a very slight degree. But I cannot leave out of account the fact, that in nature, cold does not have the least tendency to produce a dark coloration, and that the arrest accompanying extreme cold tends to produce, as a rule, pallid ill-marked specimens, with a tendency to become generally unicolorous, apparently the result oftentimes of ill-developed pigment in the scales.

With regard to seasonal forms in Britain, we may consider a few cases. *Pieris rapæ.* The early brood (May and June) specimens are much whiter with us than those of the summer brood. I think this affords one of the most striking proofs of the absence of the effect of cold on the pupal stage. *Pieris rapæ* lays its eggs in August and September, pupates in September and October (frequently in larval stage under four weeks), remains as pupa exposed to cold all the winter, and emerges in May as a pale form, frequently with the dark spots obsolete and therefore perfectly white. Eggs of the summer brood are laid in May, larvæ feed through June and July, pupate July and August, emerge August and September, the imagines being comparatively dark, having dark tips to wings, and distinct black spots. Here it is impossible for cold, acting on the pupæ, to have any effect in the direction of darkening the colour, as those exposed to the winter cold are the paler forms, and I can only suppose that the retardation in the early larval stage, aided by "natural selection," is the true active cause of the darkening, as the pale specimens, with their undersides brightly yellow are more protected in early spring, when the more common *Cruciferæ* are in flower (white or yellow) and the leaves are bright. In the summer the insects assimilate by means of their darker colour (especially greyer undersides) more closely to the darker leaves. The allied *Pieris napi* has imagines of the spring brood with the ground colour dusky, markings dusky-greyish; the summer imagines have the ground colour white, markings black. Comparing the two species, my experience points to the second brood of *P. napi* being in the larval stage longer than *P. rapæ*, and I have frequently known larvæ to live from early August until October before pupating. Although equally common perhaps, the habits of these two species are very different, and whilst *P. rapæ* is more especially addicted in its early stages to open cultivated grounds, *P. napi* prefers marshy land, ditch sides, shady rides in woods, etc., so that environment and "natural selection" undoubtedly have considerable effect. I still consider, how-

ever, that the study of the undersides is the more correct method of dealing with all our butterflies, from the point of view of "natural selection," but in these species the upper sides are comparatively a reflex of the undersides, *napi* being the darker in the early brood, and very similar to *rapæ* in the summer brood. The spring specimens of *Lycæna* appear to vary somewhat in a proportionate degree to the length of time passed in the larval stage, and the peculiar habits of the respective broods.

A seasonally dimorphic species on which I have frequently experimented is *Tephrosia crepuscularia* (*lariciaria*). The March and April broods from eggs laid in April, larvæ feeding throughout April, May, June, July, and very frequently August,[1] produce moths generally of a deep ochreous tint, varying somewhat in intensity according to the woods in which they are found (due probably to "natural selection") but still always more or less ochreous. The second brood, from eggs laid in April, larvæ feeding in May and June, produce pale whitish moths (no ochreous colour) in July.[2] I have repeatedly had a large brood, eggs all laid at one time, hatch simultaneously,—part of the larvæ feed up, pupate and emerge in eight or nine weeks producing the pale form,—whilst the remainder of the brood (under the same conditions) have fed slowly on, pupated in some fifteen or sixteen weeks, lain over as pupæ until March, and then produced the ochreous form. I have forced these autumnal pupæ, so as to obtain emergence throughout January, but they never produced anything except the ochreous (spring) brood. Now if Mr. Merrifield's conclusions previously referred to, were at all capable of generalisation, these forced specimens ought to be pale, but they never are, and I would suggest that the cause of difference is in the retarded larval conditions and the influence of heredity which makes one part of the brood grow slowly and pass the winter as pupæ before emergence. I have had a precisely similar experience with *Selenia illustraria*. Moths of the spring brood captured at large in Chattenden, have given me in the same brood (1) rapidly feeding larvæ which have emerged in July (as early as the 11th) as the summer form, and (2) slow feeding larvæ which have produced in April of the next year the dark winter form.

[1] I think *T. crepuscularia* is one of the slowest feeding species I ever reared.

[2] The larvæ from this emergence are just as slow in feeding up, and are rarely full-fed before October.

I have also bred the dark winter form after an average temperature in my greenhouse of 48° for the winter, again proving that exposure to, or protection from cold has nothing to do with the coloration. But, whilst considering the seasonally dimorphic species *Selenia illustraria* and *S. illunaria*, I cannot help remembering how much their coloration must be due to protective resemblance, and as the spring broods of these species hang, wings downward, they can hardly be detected from the dry withered leaves on the plants they respectively frequent. Their environment is so different in the summer months, that it appears to me, quite sufficient to account for the difference in colour of the two broods.

To show how close is the connection between heredity and the coloration of seasonal dimorphic forms, and the influence that the former undoubtedly has on the latter, I will quote some of Mr. Merrifield's conclusions with which I am in almost perfect accord. That gentleman writes with regard to "heredity" and "seasonal double-brooded species":— "(1). In such a species a young individual may have, and often has, a constitutional capacity for developing into either type, according to external circumstances. (2). It seems probable, that there is, from the beginning of the existence of the individual, a tendency, which may be very strong or very slight, to develop in the direction of one of the two types. (3). If there is no such innate tendency in an individual it can be imparted by external influences during the early part of its existence. (4). Where the tendency exists, it varies in strength in different individuals. In the case of some species, or some broods or individuals of some species, the tendency from the beginning is so strong that it cannot be overcome by any external influences. (5). In other cases the tendency can be overcome and converted into the opposite one, or turned more or less in the direction of it, by such influences. The *decision* as to the type to be assumed is come to before the termination of the growth of the larva, and this decision may be completely controlled in some cases by external influences applied before that period. For example, in 1887, by keeping the insect in all stages at a temperature of about 80°, I brought out four successive broods of *illunaria* in the ten months, all of the summer type.[1] (6). After larval growth is completed, no complete conversion of the one type into the other can be

[1] By bringing out four broods in one summer at a high temperature, there was no delay in larval stage.

effected; it seems clear that such a conversion cannot be made as regards size,[1] and but slightly, if at all, as regards shape;[1] and it seems that it cannot be completely made as regards colours[1] or markings.[1] This incapability as to colour and markings certainly exists as respects *illustraria*, also as regards *Vanessa levana* and *P. ajax* (Professor Weismann's experiments); and I gather that, in the cases published as to *P. rapæ*, *P. napi*, *P. pharos* and *P. interrogationis*, the butterflies from the iced summer pupæ presented some differences from the normal form proceeding from the winter pupæ" (*Trans. Ent. Soc. Lond.*, pp. 146, 147). Nothing could show better than these conclusions, how utterly impossible it is to leave heredity out of account when experimenting.

The influence of heredity in causing variation is again well recognised by Mr. Merrifield in the following:—"It will be seen that there is some individual variation *more particularly* in the pupæ exposed to the ordinary temperature, and therefore some of the colouring must be attributable to individual and presumably hereditary qualities" (pp. 137, 138); and the still further certainty of outside influences is shown in the failure to produce similar results from *tiliaria* (presumably not domesticated like the other species), "the results on the colouring, though tending in the same direction, were by no means so regular or so striking" (p. 138). "There is no doubt a strong pre-disposition, in an individual belonging to a double-brooded species, at some period of its development, towards one of the two different destinations, *i.e.* the emerging in the summer and with the summer colouring, or the lying over until the spring and then emerging in the spring colouring. The experiments lead me to think that in the species operated on by me the predisposition has become so decided in the larval stage[2] that no treatment of the pupa can afterwards entirely alter it, but, that in the early larval stage, treatment can—I do not say in all cases—either give the required pre-disposition, or, where it exists naturally, can completely reverse it" (pp. 142, 143). Here my experience with larvæ is in unison with Mr. Merrifield's. All my notes and observations on the matter, satisfy me that the larval stage is the one which is extremely sensitive to external influences.

Closely connected with heredity is the question of dark forms being brought about either by reversion or progressive

[1] I go further, and suggest not at all in either direction as most probable; certainly not, in size, shape and markings.

[2] I feel satisfied that this is the stage which is affected by external circumstances.

development. Of the "Coalescence of dark markings," Mr. Cockerell writes:—"*Argynnis bellona* var. *fasciata*. The zigzag band fused with the outward of the inner markings, forming a white band (Maynard). *Chrysophanus hypophlæas* var. *fasciatus*, Strecker. *C. phlæas* var. *fasciatus*, *Entom.* xi., 25, with fig. (W. P. Weston). *Acronycta tridens* var. *virga* (*fasciata*), with marginal band, *Entom.* xi., 24. In these cases the dark markings have coalesced to form bands, such as are normal with other species, and it is hard to consider them entirely as cases of disease. Whatever may be their nature, I think there can be no doubt that they point to the way in which bands were originally acquired in banded species, and very possibly by careful breeding from banded aberrations such as these, a two-banded race might be formed" (*Entom.* xxii., p. 99). I do not for an instant imagine these to be cases of disease, but a matter of progressive development brought about by "natural selection." Many cases occur to me. *Argynnis adippe* var. *cleodoxa*. If the silvery markings have originated in the metamorphosis of some white pigment, var. *cleodoxa* must be a decided case of progression, for the original white, represented by the silvery spots, has here become reduced to a minimum, its place being supplied by fulvous. I have a fine series of varieties of undersides of this species, in which the intermediate forms between a remarkably silvery under surface, and total absence of silver spots, are represented. *Argynnis euphrosyne* and *Melitæa athalia* frequently assume a more or less banded form. *Syrichthus alveolus* often has the pale spots reduced to a minimum, thus closely resembling some of the allied south European species; *Lycæna agestis* and its var. *artaxerxes*, vary much in the quantity of orange spotting round the wing; *Hepialus humuli* males from Shetland become ochreous like the females; ill-developed spots in *Zygæna;* banded form of *Eulepia cribrum* (if this be not indeed a true melanism); dark banded *Odontopera bidentata*, *Crocallis elinguaria*, some of the *Ephyras*, *Acidalia bisetata* (outer margin), *Acidalia aversata* (banded form), *Lomaspilis marginata*, *Lobophora lobulata*, *Larentia cæsiata*, *Scotosia certata*, *S. undulata*, *Anaitis plagiata*, *Carsia imbutata*, *Eubolia lineolata*; most of the genus *Tæniocampa*, etc., of all of which species I possess banded specimens,—all these I look upon as cases of progressive development towards a banded form, brought about or strongly aided by "natural selection"—rather than cases of heredity, disease, or

reversion. Dark vars. of *Fidonia atomaria* and *Strenia clathrata* I look upon more in the light of true melanisms.

I have given several cases of "progressive development" tending to produce more or less partial melanism, but I am quite aware that it is possible to follow out these cases in the opposite direction to that which I have done, and look on them as cases of reversion. To me, no family of moths shows such ample proof of a tendency to become melanic by reversion as our so-called "Carpet" moths, comprising the Geometrid genera—*Melanthia*, *Melanippe*, *Coremia*, *Camptogramma*, and the allied *Larentia* and *Emmelesia*. I consider the most highly developed forms of these genera to be those specimens which are practically unicolorous, white, buff, or golden, with an entire (or almost entire) absence of transverse bands. I look on the least specialised forms as those which are dark in colour, covered with dark transverse wavy lines, with but rare tendency to banded form. I will take a few specimens as types of the whole group. *Melanippe hastata* var. *hastulata* (?), found in Scotland and the Hebrides, has the broad black transverse bands broken up into narrower bands and finer transverse lines. *Melanthia rubiginata*, the type in the southern districts of Britain, has only faint traces of dark central and marginal bands: as we get further west and north, the complete central-banded form (var. *virgata*) becomes typical; in certain parts of Ireland and Scotland no other form appears to be known; in the Trossachs and other West Scotland districts, the banded form is partly replaced by an unicolorous blackish form (var. *plumbata*) with transverse lines. *Melanthia albicillata* occasionally throws a form (var. *suffusa*) parallel to *rubiginata* var. *plumbata*. *Melanippe tristata* varies locally, in some districts the whole area of the wing is covered with transverse lines, in others the central area is very distinctly banded. *M. sociata*, a distinctly banded species on a white ground, in the south, exists in the Hebrides as a species with the transverse lines on a dark ground. *M. montanata* occasionally almost pure white, frequently has the central band only represented by two darker external transverse lines, becomes more completly banded normally as we go north and west, until, in the Shetland, the ground colour becomes greyish ochreous and the wings frequently crossed by transverse lines. *M. galiata*, a distinctly white species with a dark central band (frequently pale transversely in the upper part), becomes dark grey in ground colour, and has a very much blacker band in Lanca-

shire. *M. fluctuata*, in its highest developed forms pure white (var. *immaculata*), then with a small costal spot, becomes as we travel north and west almost uniformly grey in ground colour, with the band becoming more or less complete, until, in certain parts of Scotland, the ground colour becomes greyish-black, and the central band in many specimens becomes more or less merged into transverse lines crossing the whole of the anterior wings. *Camptogramma bilineata*, in its highest developed forms, in both sexes, almost unicolorous golden yellow, transversely lined with yellowish colour in both sexes, becomes frequently faintly lined with black near costa, which develops into a more or less complete band in some (generally female) specimens, the black band becoming more typical in Scotland and affecting males equally with females, until we find that Hebridean specimens have the ground colour frequently greyish yellow with a distinct black central band, the other parts of the wing also finely lined transversely. These, I think, will offer sufficient examples for my purpose.

As I have previously stated, there seems to be but little doubt that, when the country was covered with forests, etc., the general condition of the atmosphere was more humid than at the present time, and I have before pointed out how, as Dr. Chapman has stated, in the northern and western parts of the British Islands, rocks, tree-trunks, etc., are made permanently darker by rain. Now, the species of moths to which I have just referred, all sit with their wings fully expanded, so that the whole surface of the wing must be taken into account in studying the effect of "natural selection" on the group. They rest on leaves, trunks, fences, and similar situations in the south, and hence their colour assimilates in some measure to their respective resting-places; but, in the more exposed localities in the north, they settle frequently on the bare rocks. In moist and exposed situations under such conditions, then, we should look for an approximation towards the original forms in the species, and in such, we find but few banded specimens, the prevailing forms being of a dark ground colour crossed with transverse wavy lines. That we get a gradual increase of the banded form as we come from the west and north of the British Islands, appears to be convincing proof of the theory that bands are formed by the union of transverse lines in certain parts of the wing, coupled with the suppression of the transverse lines in the other parts, in the same way that the lines forming a

central band are frequently suppressed on the inner margin, leaving only a dark costal patch. Now, if we look on the pale forms, at present typical with us, as the type, there is no doubt that the dark forms must be looked upon as reversions, and, on the contrary, if, as I assume, these dark forms have really never been actually modified, but represent the original form, then we must look on the banded forms, those with a costal patch, and forms without dark markings, as all steps in one gradual line of development, of which the latter are the higher forms. I believe at any rate, that it is impossible to separate melanisms due to reversion, from melanisms due apparently to development, as some instances may simply be the exact converse of others.

Heredity was one of the points which I suggested should not be overlooked in considering the production of melanism, but there is yet another cause of melanism that should be mentioned, and that is disease. There is no doubt that disease, especially disease brought about by continued interbreeding and the consequent gradual weakening of the race, does produce melanism in certain species. In the *Entomologist's Record*, vol. i., pp. 236, 237, I have just touched on the subject, and instanced how often crippled specimens, the result probably of diseased or weak larvæ, are darkened in colour, and specially instanced a large brood of inbred *Arctia menthastri* which were very variable in markings, and which all showed a melanic tendency. But attention has only just been drawn to the subject, and there is no doubt that more facts will soon be forthcoming. I have, at the present moment, in my possession a very long and varied series of *Cuspidia alni* bred by Dr. Chapman. This species, he informs me, he has bred and inbred for some years. The specimens were all at first quite normal in colour and markings, and whilst this was so, the progeny were interbred without difficulty. This year, the brood produced scarcely any typically coloured specimens; almost all were varieties, and much darker (some very much) than usual, but the moths failed to produce fertile ova, and the brood died out. Here, it seems almost certain, that the darkening was due to disease, brought about by "in-and-in breeding." I find also a remark by Mr. J. C. Warburg which runs as follows:—"A batch of larvæ of *Vanessa antiopa* which were under-fed, produced seven *dark* specimens of small size, about two inches in expanse" (*in litt.*); whilst of *Melitæa cinxia* he writes:—"A few small bred specimens have the black mark-

ings (especially beneath) more pronounced." Dr. Buchanan White (*Ent. Mo. Mag.*, vol. xiii., p. 149) writes:—"Frequently, melanochroic (and more frequently melanic) individuals are of smaller size than the typical form." The influence that this (disease) must undoubtedly have, in experiments which we carry out by breeding insects, must not be lost sight of altogether in the inferences and deductions we may make from the results apparently produced.

In connection with disease as a cause of partial melanism, Mr. Cockerell, writing under the title of "Suppression of light markings," says:—"*Papilio asterias* var. *asteroides*, Reak., *Limenitis arthemis* dimorphic forms *lamina*, Fab. and *proserpina*, Edw., *Apatura iris* var. *iole*, Schiff., Newman's *British Butterflies*, 72, *Mamestra persicariæ* var. *unicolor*, Stdgr. These must, I suppose, be regarded as cases of partial melanism; but Mr. H. Goss (*Entom.* xi., 73), in describing a dark variety of *Chelonia villica* from Brighton, mentions that several dozen larvæ from the same place produced the type, and suggests further that the aberration was due to a diseased condition in the larva. Now this variety exhibited coalescence of the dark markings and suppression of the light ones, which further, was more complete on the right than on the left side, so it seems certain that it had nothing in relation with such cases of melanism as *Pieris napi* var. *bryoniæ*. I am therefore inclined to separate all dark forms into two groups:—(1). True melanisms. (2). Cases of coalescence of markings due to disease. What the precise nature of the disease may be, and which forms precisely are to be classed in either group, is for future investigations to decide" (*Entomologist*, vol. xxii., pp. 98, 99). I have in my cabinet three specimens of *Vanessa urticæ*, bred by my friend Mr. H. Page, which were supposed to have been crowded as larvæ, which produced among other slightly aberrant forms, two specimens with very suffused, and one with perfectly black hindwings. Here there seems to be some distinct connection between cause and effect.

I have previously referred to the possibility of Mr. Merrifield's results having been influenced, in the case of *Selenia illustraria*, by a tendency to disease (possibly through interbreeding). How probable this is, may be seen by carefully looking at the figures of his specimens (*Trans. Ent. Soc. Lond.*, 1890, Pl. V.), where figs. 3, 6, 11 and 12 are more or less what would be vulgarly called cripples, whilst the asymmetry in some of the other specimens points exactly to the same con-

clusion. When one reads such as the following:—"It is probably owing to the greater exposure that they have a less vigorous appearance, and include a large proportion of cripples" (p. 137), when speaking of the dark and much-spotted moths; and again "The whole of the fourteen emerged in good condition, showing that I had fallen on a healthy brood; and showing also that it was a naturally dark-coloured one, and therefore, perhaps, not so well adapted to show any darkening of colour as a lighter-coloured set would have been" (p. 139), thus unconsciously linking a certain (dark) colour with health and *vice versâ*, and when one reads as results:—"Thirty-six pupæ, twelve emerged, of which seven were cripples;" "sixty-three larvæ, eighteen pupæ, four emerged, of which two were cripples;" "twenty-six pupæ, thirteen emerged, of which four were cripples, etc." (p. 142), I think some consideration should be given to the facts.

In *The Entomologist's Record*, etc., vol. i., pp. 232, 233, I referred to a paper by Lord Walsingham, in which it was suggested that the dark coloration of insects was probably due to the rapidity with which these colours absorbed heat in high latitudes and altitudes; and although I quite admitted in my remarks, that a dark colour would be an advantage, under such conditions, to those insects possessing it, yet, since a dark coloration was not characteristic of high latitudes and altitudes, the advantage due to the rapidity of absorption could hardly be looked upon as a cause of melanism. Referring to this in his Presidential Address to the Fellows of the Entomological Society (*vide Trans. Ent. Soc. Lond.*, 1890, pp. liv., lv.), Lord Walsingham says:—"Mr. Tutt, referring to a paper of my own in which I called attention to the tendency to melanism exhibited by Arctic and Alpine Lepidoptera, points out that insects from high latitudes are not generally melanic. I think I may at once admit that I had used the term 'melanic' somewhat incorrectly in this connection; what I desired to point out was the general tendency of Arctic Lepidoptera to a certain suffusion of markings, and to an increase in the proportion of dull or dingy scales calculated more rapidly to absorb heat than the purer white of more southern varieties. Such a tendency will, I think, be admitted to exist, but I am aware it is far more conspicuous in many insular and Alpine districts; and, while I cannot agree that the arguments put forward in that paper are in any way undermined by this admission, or that the advantage secured to the species by the

development of colour capable of rapidly absorbing heat has been in any way disproved, I am quite willing to accept Mr. Tutt's assertion that melanism does not habitually occur unless lower temperature is accompanied by increased humidity; qualifying the acceptance only by suggesting that anything which would have the same effect as increased humidity in diminishing the action of sunlight would probably be found to produce the same results."

I am very pleased that Lord Walsingham thus far admits the justice of my criticism. I quite agree with him that the arguments in his "paper are in no way undermined" by his admissions, except so far as these arguments seek to bear out that dark coloration, owing to the rapidity of heat absorption is a *cause* of melanism; the arguments brought forward to show that dark coloration is in certain cases *an advantage* to the insects possessing it are certainly as potent as ever. I do not, however, altogether agree with his suggestion "that anything which would have the same effect as increased humidity in diminishing the action of sunlight would probably be found to produce the same results," unless indeed, it would have the same effect in influencing the surroundings, and thus promote the same course of development by "natural selection." For example, snow-clouds in the higher open Arctic plains of Europe, Asia, and America, obscure the sun's rays for the greater part of the year, for a much longer period of time certainly than do the rain-clouds, etc., of our own Islands; again, Nature shuts out the light of the sun from the greater part of these areas, with the exception of a very few hours, for the whole period from November to February, and yet the insects from these open Arctic plains show no tendency to melanism, as I have over and over again repeated, when attempting to show that cold in these regions does not produce melanic forms. Here we have insects brought up naturally with a minimum of sunlight affecting them, but Nature does not here produce melanic forms.

In a previous paragraph (*Entomologist's Record*, etc., vol. i., pp. 89, 90) I stated that I failed to see how the absence of sunlight could in any way account for any form of melanism or melanochroism in lepidoptera, but Lord Walsingham, as may be gathered from the preceding paragraph, appears to think differently, and opens up a new path in this direction. He says:—"There are many local circumstances which cause an interference with direct sunshine; dense forests occur

where the full rays of the sun never penetrate, clouds and mist accumulate around the summits of high mountains, fog and smoke envelop the districts immediately surrounding our manufacturing towns, islands in a temperate climate are subject to condensation of moisture and sea-fogs, and under all these circumstances dark varieties of certain species are known to occur, although the same species when found under different conditions more favourable to the action of light, are usually less intensely coloured. But if moisture is to be taken as a direct, rather than an indirect, cause, we should expect to find melanic variation occurring in the swamps of Tropical Africa, in the forests of the Amazon, on the banks of the Mississippi, and in many other damp climates, even within tropical regions, and I am not aware that this is the case. It has been pointed out that no variation in the direction of melanism has been found to occur in dry open level tracts, however far north these may be; and this fact, although it tends to show that cold is not by itself a potent cause of such variation, is perfectly consistent with the theory that diminished sunlight exercises a certain influence upon the direction in which colour may be expected to, and does, vary. The only conclusion that I would venture to maintain, in regard to this very wide subject, is that variation of colour in the direction of melanism, melanochroism, or such suffusion of markings as can appreciably affect the rapidity of heat-absorption by the wing-surface of any insect, is very generally proportionate to the degree of interference with the direct action of sunlight caused by climatic or other conditions to which the insect is exposed, and that such interference may have a more or less immediate, or only a very gradual effect, upon the variation of species. We have yet to ascertain how far such effect is assisted by a process of natural selection, and whether such natural selection is exercised for the sake of protective resemblance, or for the advantage derived from an increased power of heat-absorption, or whether it is produced by the direct chemical action of the visible or invisible rays of the solar spectrum.

"I sincerely wish that Mr. Poulton or Mr. Merrifield, or some of those gentlemen who have time and opportunity for studying this question, would carry out an experiment, originally put into my head by Sir John Lubbock, viz., the rearing of some of our variable GEOMETRIDÆ, such as *Gnophos obscuraria*, *Cidaria russata*, or some of the species of *Melanippe* or *Hybernia*, guarding them in the larval and pupal stages by

hyposulphide of carbon or bichromate of potash from the action of the ultra-violet rays of the sun, which are supposed by many to have such a strong chemical effect upon the colouring matter in the scales, or on the chlorophyll in the pigment of the larvæ. If it were found that under such conditions any appreciable change could be made in the percentage of dark as compared with paler varieties, it would go far to prove that melanic variation cannot be attributed to the direct influence either of cold or moisture, but rather to the indirect influence of the two combined; and the facts, so far as they are at present known, point, I think, somewhat forcibly to this conclusion" (*Trans. Ent. Soc. Lond.*, 1890, pp. liv., lv.).

These remarks suggest many ideas, and open up new standpoints from which the subject may be discussed, and the following suggestions at once presented themselves to me as I heard the address.

Although I certainly look on moisture rather as an indirect than a direct cause of melanism, I am not inclined to discount the probability of "melanic variation occurring in the swamps of Tropical Africa, in the forests of the Amazon, on the banks of the Mississippi, and in many other damp climates even within tropical regions." To a great extent our knowledge of the fauna of these districts is more or less confined to the larger and more conspicuous of the lepidoptera, especially diurnal lepidoptera, whose habits would render them less susceptible to these influences, their environment leading them to vary mainly in other directions. When the nocturnal lepidopterous fauna of these districts is as intimately known as our own, I feel satisfied that numerous cases of melanism will come to light proving that the combined influences (direct and indirect) of moisture and "natural selection" are of general application.

Lord Walsingham's idea as to the action of the actinic rays of the sun in producing colour, and the absorption of certain of these rays by clouds, etc., as a probable cause of melanism, is quite new to me, and has proved of great interest, although at present I cannot see my way clearly to accept the suggestions to which the idea leads. I suppose that since the solar spectrum consists of heat, luminous and actinic rays, we may perhaps assume that a lessened amount of luminous rays, after having filtered through clouds, etc., is accompanied by a fewer number of actinic rays, and that the clouds may have a

corresponding absorptive and diffusive[1] power over these rays as they have over the luminous. Considering, too, the well-known phenomenon of the decomposition of carbon dioxide by the chlorophyll of plants in the presence of water, by the direct action of the actinic rays, there seems a superficial reason for considering that these rays may have, as is observed by Lord Walsingham, some action on the chlorophyll in the pigment of the larvæ. But I would ask what effect (direct or indirect) can the actinic rays of the sun have on those larvæ which have not the slightest trace of chlorophyll in their pigment. NOCTUÆ larvæ, which feed on roots and never come under the influence of sunlight at all; wood-borers as the *Hepialidæ*, *Sesiidæ*, etc., *Prays curtisellus;* richly-coloured, densely-haired larvæ as *Arctiidæ* and *Acronycta*, and so on, many of the species of which show melanic variation. I am afraid I must own that at present I am unable to see any connection between cause and effect in this direction, although it certainly is a matter worthy of the most careful consideration.

Although I thus still venture to differ from Lord Walsingham, I must candidly confess that his remarks have modified my previously formed opinions, and that I should not now feel inclined to give the probable action of sunlight the short shrift I gave it in a previous paragraph.

Although not directly bearing on British lepidoptera, I referred (*Entomologist's Record* etc., vol. i., pp. 122–125) to an article by Mr. W. W. Smith, of Ashburton, New Zealand, "On the Variation of *Argyrophinga antipodum*," to show that my theory of excessive moisture producing melanism, and *vice versa*, was of general application. With regard to this, Mr. Smith now writes:—"The last three exceedingly hot and dry seasons in succession in New Zealand, which have been more severely felt on the plains of Canterbury, have been productive of remarkable colour variation in some species of lepidoptera, the variation in all cases developing in the form of much paler colours. The variation of *Chrysophanus boldenarum* was recorded by Mr. R. W. Fereday, of Christchurch, N.Z., twelve years ago, who published two papers on "The Genus Chrysophanus in New Zealand" (*Transactions N. Z. Institute*, vols. ix. and x.). Mr. Fereday, in the papers referred to, figured the type of the

[1] The absorptive and dispersive powers of aqueous vapour on the ultra-red (or heat) rays of the spectrum are well known: see Ganot's *Physics*, par. 959. It becomes certain, therefore, that the influence of vapour is to increase the quantity of obscure (ultra-red or heat) rays, and to lessen the luminous and ultra-violet (or actinic) rays, in proportion to the increase in the ultra-red.

species together with several well-marked varieties, and gave an interesting account at the time of the known habits of the genus, in the same volume (x.). Mr. A. G. Butler published a valuable paper on N. *Z.* Rhopalocera. Mr. Butler's paper was also accompanied with figures and descriptions of both sexes of all the known species. The figures given by Mr. Butler, although smaller than the majority of specimens, are typical of the species. Apart, however, from the types and varieties figured by these gentlemen, there are many other well-marked varieties occurring, besides a very striking and general variation not equalled by any other species in the genus. Although *C. boldenarum* is in ordinary seasons a very variable species, it has never previously varied to the extent it has done in the present season. I have already referred to its variation in a melanic direction in humid seasons (*Entom.*, xxii., 38). But last season, and more so in the present one, the variation is in the opposite direction, several of the female specimens exhibiting a decided tendency to gynandromorphism, and the males an abnormal arrangement and development of paler colours. The cause of these phenomena is, I think, easily understood. For three years there has been an exceedingly light rainfall on the Canterbury plains, and that, at long intervals, while the weather has been exceedingly hot and dry, and it was accompanied for several months this season with hot north-west winds, the blighting effects of which are well known and dreaded by the settlers. It would therefore appear clear that the absence of moisture in all stages of development during several generations of the species, is the chief factor in developing the pale phase of variation. These remarks apply with equal force to numerous other species of both diurnal and nocturnal lepidoptera I have collected here this season, as all species (some of course more than others) exhibit a decidedly lighter deviation of colours from the normal types.

"By way of adding a few facts in support of these remarks, I may state that even in the most favourable seasons, lepidoptera collected on the plains here are invariably paler than individuals of the same species occurring on the lower hills or in wooded districts near the Ranges. The rainfall is in all seasons much greater on and around the base of the Ranges than out on the open plains. The higher we ascend the Alps the more humidity we meet with, and the greater the darkening of the lepidoptera, until we reach the summit, when they

become perfectly black. *Percnodaimon pluto*, *Erebiola butleri*, and occasionally *Ltathmonyma hectori* are cases in point, and doubtless many more species that yet remain to be discovered. Most entomologists will naturally suppose that all mountain forms of lepidoptera are endowed with a more hardy constitution than other or allied species occurring at lower altitudes; but the cause of melanism at high altitudes is not so much a question of cold or heat as its great advantage over other colours in a wet or humid region. Doubtless Lord Walsingham's theory of the greater absorption of heat by dark-coloured lepidoptera would occasionally be advantageous to certain melanic forms, as well as in a protective sense; but, as Mr. Tutt has shown (*Ent. Record*, vol. i., 232), it leaves the origin of melanism wholly unexplained. Again, we know that melanism is prevalent in some seasons that are at the same time both mild and moist. A certain degree of moisture is necessary in the egg, larval, and pupal stages to perpetuate the typical colours of a species, and we know it is only in exceptional seasons or under exceptional conditions that lepidoptera vary most. Still, we must allow that in hot, dry seasons, the colours of many species of lepidoptera are rendered paler, or obsolete, by the sun's absorption, or bleaching of the colours affected; but we are not so much interested in this question at present as we are in the origin and causes of melanism. One remarkable instance which would appear to oppose Lord Walsingham's theory occurs in the habits of *Nyctemera annulata*, a jet-black diurnal moth with two white marks on each forewing and one on the hindwing. The moth in a natural state, or when bred, almost invariably emerges from the cocoon and flies about on dull and drizzly days. It may sometimes be seen soaring in the cool early morning, but at all times it shuns the hot sunshine. It would therefore appear that the theory of absorption of heat will not apply in all cases. The imagines of our dark Alpine species appear in January and February, which are the clearest and hottest months in the year. Nothing is absolutely known of their economy and habits, yet we may surmise that the eggs are deposited soon after emergence, and the larvæ feed up in March and April. In the latter months the autumnal rains set in, which are greater and more prolonged on the Ranges than in the lower country. It must be admitted that larvæ feeding on high altitudes are subjected to a lower temperature; but it will also be observed that they are subjected in our climate to a much greater degree of moisture than other or

allied species existing on the lowlands. About the end of April or beginning of May, the snow begins to cover the higher parts of the Ranges, and continues, more or less, to fall until September, when the warm north-west wind generally begins to blow and melt it off the Ranges. The pupæ thus deeply buried in snow through the winter, would experience no more cold on those high regions than other pupæ exposed to all weathers on the plains, on which snow scarcely ever falls. The north-west winds which begin to blow in September, are at first slightly chilly, owing to their traversing the snow-fields of the Alps. As the snow lessens in quantity, the wind blows hotter and drier across the plains, causing the snow-fed rivers to flood, although the wind is cool before it impinges on the western summits of the Ranges. It absorbs great moisture in crossing the snow fields, which is frequently condensed in heavy rain showers over the lower hills and downs, and along the bases of the mountains; here the lepidoptera are invariably darker than on the open plains, where the warm north-west rains rarely ever reach. The physical features of the country I have described, namely, high mountain ranges and extensive plains occurring contiguously, differ greatly from any districts as yet mentioned by Mr. Tutt, where he states melanism occurs. Nevertheless the cause of melanism and melanchroism appears to be identical in all cases. The very reverse meteorological conditions of climate are frequently observed here on the same day, for, although the day may be dry and hot on the plains, the Ranges are at the same time enveloped for days together in dense aqueous clouds condensing great moisture (see remarks by Professor Schöyen of Christiania, quoted by Mr. Tutt, *Ent. Record*, vol. i., 53).

"The elucidation of any cause producing variation in animals requires very careful study, and any theory advanced to account for the variation of certain groups or species, should be done with extreme caution, but the main line of argument on which Mr. Tutt continues his series of papers on 'Melanism and Melanchroism in British Lepidoptera' is well supported by the same natural causes and effects in New Zealand. Where the variation of any species in a given direction develops under certain conditions, we may safely assume that it is due to certain natural causes operating in the direction the form of variation assumes. The result of my experience with variable New Zealand lepidoptera so far favours the moisture theory of melanism" (*In litt.*, Feb. 25, 1891).

The earliest explanations of melanic forms were generally physiological or had a physiological basis, and the supposition was, that the exciting cause had a phytophagic origin, and that the larva was affected in some unknown way by the chemical elements in its food, or by certain external substances taken with its food. Thus peaty soils, soils impregnated with iron, leaves covered with soot, etc., have all been supposed in some mysterious manner to have affected lepidoptera in the direction of melanism. Now it is well known that a plant takes the same inorganic and organic substances for food wherever it may be grown, and that the proportions of these substances vary only in the very narrowest degree. Any deficiency of a food-substance in the soil is at once visible in the vegetation, and stunted growth is the first result of deficiency. Total deficiency means "barrenness" for any plant requiring the absent substance. Now, whether a plant be grown on a "sandy" or a "calcareous" soil, the chemical analysis will prove that the plant has its tissues made up of the same substances and in almost exactly the same proportions on either soil. I would ask therefore:—How can the food influence colour? If a soil has large supplies, in a soluble condition, of all the necessary foods that a particular plant requires, we get a luxuriant growth. On the other hand, if a soil has but a small quantity of the available foods, then that particular plant will be stunted and sparse in its growth, but the plant on either soil will still be composed of the same chemical substances. Phytophagic varieties in this way would generally be variations in size, as in the case of the before-mentioned species, *Hypsipetes elutata*, and not variations in colour.

My friend, Dr. Chapman, I know, always connects the beautiful var. *rosea* of *Cuspidia tridens* with cherry (*Ent. Rec.* ii., p. 31), but I do not know that he has formed any idea as to the way in which the colour may be produced by this food, and I simply mention the fact to show that other lepidopterists consider that food does influence colour. Exact experiment on the matter is undoubtedly needed.

There is one way, however, in which food may more or less indirectly produce a dark coloration. A plentiful supply of succulent and nutritious food will generally produce insects of large size, whilst an insufficient supply of food, or even a plentiful supply of innutritious food will act in the opposite direction. In these specimens, however, the smaller size is often accom-

panied by the markings being pressed much more closely together, thus giving the insect a darker appearance. This is especially the case if the ground colour be pale and the markings dark, for then the area of the pale ground colour becomes lessened, and the darker markings are developed in a smaller space. It becomes hard to dissociate this form of development from an indirect tendency to disease, or rather to a change of constitution as Dr. Chapman terms it; but all instances of this kind appear to be only very indirectly caused by food, and will be found, generally, to be governed by the environment of the particular species.

In the *Ent. Mo. Mag.*, vol. xiii., p. 132, we find Mr. Birchall attempting to show us that "there are great constitutional differences in animals and plants, which are correlated with differences of colour," and he refers to chaps. xxi. and xxv. of Darwin's *Animals and Plants under Domestication*, where he states it is shown "that black and dark-coloured animals escape many diseases, are less liable to the attacks of parasites, and will stand changes of temperature, which prove fatal to the lighter coloured varieties." Unless in the case of true albinisms, I should challenge this statement. We must bear in mind that true "albinisms," which must not be confounded with "leucochroisms," are generally looked upon by scientific men as being a certain form of disease, or rather due to certain conditions of disease, and we can readily understand that, if this be so, the white varieties would necessarily be constitutionally weaker than their darker brethren. Mr. Birchall then quotes at length from Mr. A. R. Wallace's recent *Address to the Biological Section of the British Association at Glasgow*, but the conclusions of Mr. Wallace are more or less open to question, and I doubt altogether, both his conclusions and the one at which Mr. Birchall arrives:—"As it thus appears certain that greater strength of constitution and more powerful and acute perceptive faculties are, from some yet unknown cause, associated with dark colours in the vertebrata, may we not presume that insects are subject to the same law, and that dark varieties of lepidoptera are able to spread and increase under adverse conditions, whilst the lighter coloured types fail to do so, and are consequently eliminated in the struggle for life, and that the occurrence of melanic forms may be thus reasonably explained as a simple case of the 'survival of the fittest'?"

Taking man as the highest of the vertebrata, such a conclu-

sion as this seems to force us on the horns of a dilemma. Either the pale Anglo-Saxon race, which has shown its strength over all other races, and has fixed itself as the predominant race of the world, must still be looked upon as inferior to the races under its sway, or we must look upon the predominant race as a great exception to what above is called "the rule," although I do not follow out the application of "the rule." Dr. F. Buchanan White, dealing with this question (*Ent. Mo. Mag.*, vol. xiii., p. 149), writes:—"That melanochroic (or melanic) insects are peculiarly favoured with stronger constitutions and more acute senses, there is not, I think, any reason for supposing. Frequently, in fact, melanochroic (and more frequently, melanic) individuals are of smaller size than the typical form." I think decidedly, variation in size in lepidoptera, has its origin essentially in phytophagic causes, and that where melanic forms are small, the size is due to phytophagic, the melanism, to other causes. Mr. S. Radcliff Fetherstonhaugh also discusses Mr. Birchall's conclusion in the same volume of the *Ent. Mo. Mag.*, p. 215, and writes:—"Mr. Birchall quotes from learned writers, who assert as fact that dark coloured animals, from the lower orders up to the superior animal, man, have advantages in freedom from disease, less liability to parasites, superior acuteness of the senses, etc., which their paler coloured fellows do not possess; I must say, I do not see any foundation for this doctrine. In the races of men it certainly does not appear to hold good, as the fair-haired Saxon is able to hold his own physically and intellectually against the darker races. The single instance in which the rule holds being the albino in all animals, but this is, after all, a diseased type. If Mr. Birchall's theory of survival of the fittest be true, and that the darker races in insects, animals, and the superior animal, man, are the fittest, the inevitable conclusion to which it points is, that the darker forms in insect and animal life, and the negro in man, would, after so many ages of 'natural selection,' largely predominate in the world, the contrary being, however, the fact." This is exactly my own position with regard to this subject, and I think that such a theory is altogether unsupported and entirely contrary to fact.

But then Mr. Fetherstonhaugh, to account for melanism, propounds another physiological theory, which is apparently very far-fetched, but which it would be altogether out of reason to assume has no effect. He says:—"Dr. White appears to reject Mr. Birchall's notion that cold, damp

climates, with the absence of sunshine, may be the cause of the origin of variations of colour, and suggests meteorological causes."[1] He then goes on:—"May we not then very easily suppose that variations of colour in insects may be, so to speak, accidentally produced by external objects present to their acute vision during the process of generation, and this may occur again and again? It seems to me a less far-fetched theory than to assert dogmatically that dark-coloured insects are endowed with stronger constitutions, and are therefore perpetuated by natural selection."[2] This suggestion of Mr. Fetherstonhaugh is supported by my friend, Mr. Tugwell, in the *Ent. Mo. Mag.*, vol. xiii., pp. 256 and 257, where he writes:—"I am far more inclined to believe in the suggestion of Mr. S. R. Fetherstonhaugh, that the change is produced by a vivid impression of the colour of the surrounding objects on the female insects during the *period of generation*, and a strong tendency to reproduce in their progeny the same colours among which they themselves have lived; and, as we know that this phenomenon of nature has been accepted, we may fairly be permitted to consider it as a probable cause of melanism or leucochroism under suitable conditions." Mr. Tugwell then refers to manufacturing districts, London and the New Forest, in illustration of his idea, and gives as examples,—*Boarmia rhomboidaria* var. *perfumaria* and *Gnophos obscuraria*. He then goes on to say:—"Aberration of colour in an insect may be occasional and purely accidental, but when we see, not single specimens, but a constant and invariable prevalence of this harmonizing with the surrounding soil, I cannot but think that the cause may be mainly the powerful impression of surrounding objects on the female during the all-important period of life, *viz.*, that of propagation, coupled with an instinctive provision for the protection of its future progeny; at any rate, the subject merits a further and deeper investigation." I do not know that this theory has ever been carried further, and I am inclined to think that Mr. Tugwell is now more in favour of the "natural selection" theory from Dr. White's point of view than any other. I think, myself, with regard to this, that it would account for very few cases

[1] This is hardly my reading of Dr. White's paper. He rejects parts of Mr. Birchall's paper, certainly, but can scarcely reject "cold and damp," etc., when he suggests "meteorological causes."

[2] It must be noted that Mr. Birchall deals with "natural selection" in a physiological and constitutional way, Dr. White as selection from protective causes.

of variation, and then only such as appear to me to be better explained in other ways.

I have previously noted that other views, based indirectly on physiological causes, have been brought forward to account for melanism. With regard to these, Mr. Nicholas Cooke (*Entomologist*, vol. x., p. 94) writes, concerning the large quantity of smoke, fumes, etc., which of late years have been developed in Southern Lancashire:—"This may have effected a change in the climate, as well as deposited on the leaves of trees and foodplants of *Lepidoptera*, matter which may possibly cause some white insects to become gradually black, through being swallowed by the larvæ with their food." This, I believe, would have no effect on the larvæ themselves, as far as colour is concerned. Phytophagic varieties are, as I have before repeated, mainly[1] those of size, and I fail to see, on ordinary physiological grounds, how the colour could be affected in this way. Dr. White also writes in a very interesting paragraph on this subject (*Entomologist*, x., p. 128):—"In the same way, the foodplants may, on certain soils, affect the colours of the insects; but if it is so at all, it is probable only in a few cases; the influence of the foodplant upon the colours of insects seems not to be very great." Mr. Prest, dealing with the same subject (*Entomologist*, x., p. 130) writes:—"I do not think that either chemical fumes, or coal-smoke can have any influence in our district of Yorkshire." He then runs through a list of species which are melanic in Yorkshire, and writes:—"From these facts I form my opinion that soil in its action upon the foodplant has more to do with variation—heredity or otherwise—and melanism, than either chemical fumes or coal-smoke." At p. 131, Mr. E. K. Robinson looks upon the foodplant as being the main agent, and instances the *Leucaniidæ* in marshes, entirely overlooking the protective resemblance these species bear to the nodes on the reeds, etc., on which they rest head downwards. He also states as a fact that "food in a semi-withered or dry condition produces moths of small size; for *Amphidasys betularia* thus fed becomes in a few generations completely black; and by the same means the proportion of black to white in *Abraxas grossulariata*, and of brown and black to white and red in *Chelonia caia*, is largely increased." I do not know that there is the slightest founda-

[1] Unless the species feeds on different foodplants which have a very different chemical composition.—J. W. T.

tion in fact for supposing that any of the three species mentioned are influenced directly in the way suggested by Mr. Robinson, nor does he offer any further proof of his statements, other than thus giving most positively his own opinion on the point. He sums up his arguments as follows:—"(1). A large percentage of water in the tissues of the foodplant tends to produce a large, pale variety; while small, dark specimens result from dry and stunted food. (2). Resinous, and other strong vegetable properties, produce distinct brown markings. (3). Chalk soils produce a chalky white or bright colour in insects: conversely we should expect rich loamy soils to be haunted by deeply-coloured moths. (4). A cold climate, or the gloom of damp dark woods, causes a bleached appearance and general absence of distinct markings."

These conclusions are all more or less open to very serious objection. In No. 1, it has never been proven in the slightest degree that succulent food produces pale, and dry and stunted food produces dark varieties, other than in the direction I have previously pointed out with regard to the relation that colour bears to comparative size. This is assumed practically by Mr. Robinson, who associates "large" with "pale" and "small" with "dark." Nos. 2 and 3, I consider a simple outcome of the most elementary law of "natural selection," for the dark brown colour of fir-feeding species is of the utmost necessity for protection, when such species are resting on the dark fir-trunk, *e.g.*, *Boarmia abietaria*, *Retinia resinana*, *Thera variata* and many others; and the cause of the occurrence of pallid species and varieties on chalk needs no further discussion. That part of No. 4 referring to cold has already been dealt with, and "bleaching and general absence of distinct markings" have been proven to accompany a cold climate, but the part referring to "the gloom of damp dark woods" is purely theoretical and unsupported by fact, *vide* what has been previously written concerning the Huddersfield and Derby melanic varieties.

Still continuing the discussion relative to food as a cause, Mr. Cooke (*Entom.* x., p. 152) writes:—"The extraordinary dark varieties in Scotland cannot have been caused by smoke or chemicals, but they are, as far as my experience goes, produced in black bog or peat soil, which, I suppose, contains a large amount of carbon; and this may have the same effect on the caterpillars, through the tissues of the foodplants containing more carbon than in other situations, as when the

caterpillars eat the carbon in the form of soot along with their food; and again, although the country immediately around York is purely agricultural, yet I am satisfied there is a sufficient amount of soot deposited on the plants to affect the colours of the Lepidoptera." Here again the actual fact of the dark varieties being produced "in black bog or peat soil" points overwhelmingly to the influence of "natural selection" rather than that an excess of carbon in the soil, after assimilation, first, by the plant to form its tissues, and then by the larva when it eats it as food, should act in some unknown way on the larva and thus affect the colour of the moth, in the final stage of its existence.

I have gone thus fully into what has been written concerning "this physiological aspect of melanism," but I think there has been very little written which will strike practical lepidopterists as having any value in causing us to look on melanism generally, as a result of phytophagic variation.

Having now considered this subject from many points, I think we may safely assume that the general cause of melanism is to be sought for in meteorological influences, and that this cause must act in some way on the larva. It is during the larval existence that the insect is constitutionally strengthened or weakened, and that it is during this period that some active exciting cause acts, is evident, otherwise we should not get a sudden development of dark forms in certain seasons. Mr. J. Jenner Weir writes:—"You are quite correct about the colour of the wings being determined by the condition under which the larva feeds. This is quite true of the horeomorphic species of Rhopalocera in India, Java and other Eastern districts" (*in litt.*), *vide* also *Entom. Record* etc., vol. i., pp. 30 and 31. Mr. Merrifield—as I have pointed out before—also found the larval, the stage most affected by external conditions. I think we may safely assume that the larval, is the stage in which the melanism-producing factor exerts the greatest influence.

To account for this, no theories have as yet been advanced worthy of the name. I have two, however, one written by Mr. T. D. A. Cockerell, the other by Dr. Chapman, two of our most original and thoughtful lepidopterists. Mr. Cockerell assumes what I have proven, that the change is accompanied by a change in humidity. He then supposes that moist air may absorb some melanism-producing product of decomposition, and then he adds:—"A moderately moist air in a town

might produce the same effect as very moist air—say, in Scotland or elsewhere; thus if m = moisture and p = the melanism-producing product, then :—

2m + 1p might have the same effect as 1m + 2p.

That is supposing there to be an excess of moisture (2m) which held a certain amount of melanism-producing product (p), as might be the case in the very moist humid districts, then it might have the same effect as twice the melanism-producing product (2p) in half the quantity of moisture (1m). If this were so, we should in effect get the same results in the wet districts of Britain as in those less humid districts containing more of the melanism-producing product." Mr. Cockerell then goes on :—"If we assume that 1m absorbs 2p, the products being thus—

$$2m + 1p = m\ p$$
$$1m + 2p = m p_2$$

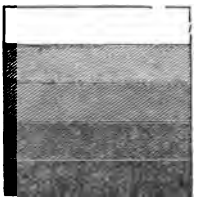
$m\ p_2$ (the town effluvium) is here stronger in p than is the $m_2\ p$, but it is more diluted with air (on the supposition that the air has less moisture, and therefore can carry less p); so that if 2 (m_2 p) is diluted in 1 cubic foot of air, then there is $m_4\ p_2$ to 1 foot of air; and also if 2 ($m\ p_2$) is diluted in 2 cubic feet of air, then there is $m\ p_2$ to 1 foot of air. That is, each foot of air contains the same quantity of p.

This is perhaps badly put, but you will (I think) see what I mean." I think the idea is very well put, and anyone who understands the simplest forms of chemical equations, will see what Mr. Cockerell means. It must be remembered, however, that this is purely theoretical, although it is quite warranted by the facts at our disposal.

I must say that, ingenious as this theory is, the following explanation by Dr. Chapman appeals much more strongly to me. It shows how completely Dr. Chapman has grasped the subject. As is usual with his writing, it is so clear that any addition to or subtraction from the original would do harm instead of good. He writes:—"I have no doubt my theory (*Ent. Mo. Mag.*, vol. xxv., p. 40) is correct in a large number of cases, but as to the ground which it actually covers, I have not made observations enough to make even a guess. Mr. Merrifield finds that delay in the larval (or pupal ?) stage produces darker specimens[1]—cold and wet produce such delay. It occurs to me to inquire whether there is any correlation

[1] To wit, the early broods of *Tephrosia crepuscularia*, *Selenia illustraria*, *S. illunaria* and *Pieris napi*.—J.W.T.

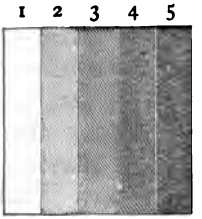
between these facts. That is, dark coloration is advantageous to a species when living in a damp and cold climate, and will therefore no doubt prevail by "natural selection." Will the individuals of a species, that is, or has been so circumstanced, acquire a habit of responding immediately in coloration to any change in the special climatic circumstances in which it is placed? A species varies, say, in colour. Let us arrange it in diagrammatic form accordingly:—



DIAG. 1.

"Then in a wet season batch No. 5 (owing to its darker colour) will have an advantage if the species rests on trees, rocks, etc., No. 4 a greater advantage than No. 3, and so on; and *vice versa* in a dry season (*vide* Diag. 1).

"Now let us suppose the same species to vary, independently in its relation to dryness and damp, constitutionally. Let us arrange it again diagrammatically (*vide* Diag. 2), in order thus:—



DIAG. 2.

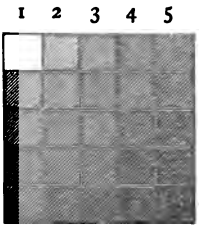
"Then the individuals of No. 1 thrive best when dry weather pushes them on rapidly, but die in wet. No. 5 gets on well and strongly in damp weather, but dies in dry (These I mean as tendencies, of course). Now if we superpose these diagrams (*vide* Diag. 3), we have a set 1–1, that is doubly favoured by hot weather, *viz.*, is *pale* in colour and suits the weather constitutionally; another set, 5–5, is doubly suitable to damp weather, *viz.*, is *dark* in colour and suits the weather constitutionally; the occupants of the other squares are in intermediate positions. In a variable climate, like that of the British Isles, it is obvious that, in some seasons, 1–1 would be highly favoured, in others, 5–5; whilst the others, 1–5, 2–3, 3–2, 5–1, etc., would always be at a relative disadvantage, and we should in course of time have a race consisting of 1–1 and 5–5, freely crossed together. In a series of dry seasons, 1–1 would predominate, after a series of wet seasons, 5–5.[1] Now what would the effect of 1–1 and 5–5 crossing together be? Chiefly, probably, they would result in a race indistinguishable from 3–3, but they would possess the potentiality of readily, under suitable conditions, evolving 1–1 or 5–5, which 3–3, if bred true in an

[1] I leave out of view, for the sake of simplicity, the fact that 2–2, 3–3 and 4–4 would also largely survive; 1–5, 5–1, and, to a large extent, 4–1, 1–4, 2–5 and 5–2 would disappear.—T.A.C.

uniform climate would not possess. Each individual is, though to appearance 3–3, really nothing of the sort, but a combination of 1–1 and 5–5, capable by selection of being easily sifted apart, and the pure race of, say, 1–1, evolved in a few generations, whereas it would require a long period to do so from a pure race of 3–3. Now the question I ask is this :— Will it result also that the 1–1 element and the 5–5 element in each individual exist in a so far independent state that if placed in circumstances favourable to 1–1, it will predominate in that individual, or, if in circumstances favourable to 5–5, the 5–5 element will assert itself during the life of that individual? Experiment shows that, as a matter of fact, some such reaction does occur in certain species, the point is, has the possibility of such a reaction arisen in the manner indicated?

"Now, would the action of these two tendencies developed under corresponding variations of weather, tend to produce a physiological correlation between them in course of time?

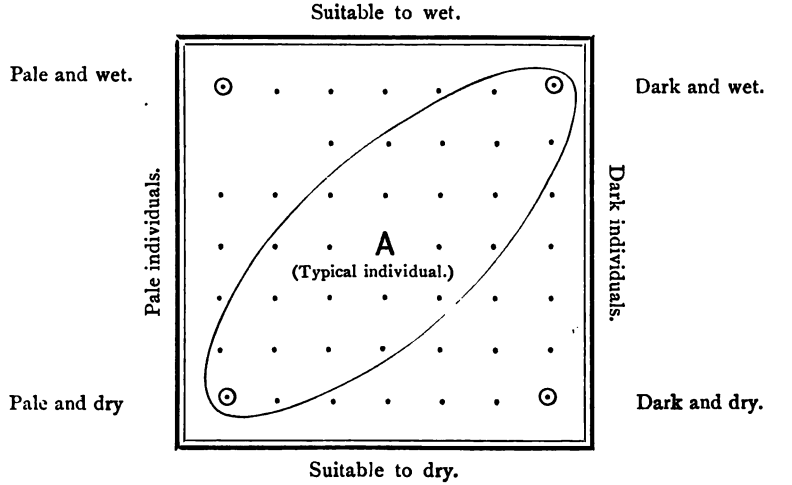
"Diagram 3 illustrates hypothetical race varying independently in colour, and constitutionally with regard to climate as suggested above.



DIAG. 3.

"Or let us go over the origin of damp producing varieties in this way :—Suppose we have a race of Lepidoptera that has not yet been influenced in this way (as a matter of fact, there is probably none that has not been so influenced over and over again), and assume, as is no doubt true more or less of all races of Lepidoptera, that it possesses (1) a tendency to vary to lighter or darker, and also (2) a totally independent tendency to vary in the way of adaptability to a drier or moister climate ;—then a dry climate or season will preserve pale vars. and those suited to dry conditions, and a wet season or climate the dark vars. and those suited to wet conditions; and *à fortiori* those individuals will have the best chance that combine suitability to climate in constitution and colour, and these will appear more numerously in the progeny by the crossing of those suited by constitution and those suited by colour, till at length these two variabilities will become correlated; the further step that the wet (or dry) season (or climate) shall elicit the latent adaptability to it, and with it the corresponding colouring, is not quite so logically

evident, but I think it follows. We want really to know whether exposure to wet (or cold) *makes* dark vars. or only selects them. I do not know that the evidence of its *making* them is yet very cogent; but I think, if two kinds or sorts of variability are thus made to occur together for countless generations, such an effect might result. But it may be merely that in a wet season dark specimens survive and are found—in a dry season pale ones do so. I think my black *Diurnæa fagella* resulted from a succession of seasons, not from one season. The following diagram may make my idea more intelligible :—



"Diagram[1] illustrating hypothetical race varying independently in colour and adaptability to climate. Individuals may be arranged in a square as above. When subjected to variations of climate, the race will take the form (compared with the original hypothetical race) shown by the curved line."

Nothing, I think, can be clearer than these ideas of Dr. Chapman, and there is no doubt they will direct attention to the subject in a different way to which it has ever been directed before. We all know of the existence of melanism; we have all seen that it is intimately connected with humid areas, and that similar conditions produce almost identical results in our own country, on the Continent, and in New

[1] In this diagram 7 dots are used each way. Had 5 been used each way, the numbers in the previous paragraphs could have been applied direct to this diagram. As it is, the same method of reasoning is applied, but A (the typical individual) becomes here 4–4 and not 3–3 as in the previous diagrams, and so on with the other numbers.—J.W.T.

Zealand. But no attempt has, I think, before been made to point out the exact way in which the active factors producing melanism, work. For myself, as may be gathered from what I have before written, I have no faith in the results of the so-called temperature experiments of Mr. Merrifield, nor do I yet accept the suggestion that cold produces melanism, but the slightest modification of Dr. Chapman's theory to exclude cold and substitute moisture as the active agent in the production of melanism would satisfy my ideas exactly. It must be owned that we are simply on the threshold of this all-absorbing and interesting subject, and in a very short time new experiments may be carried out and new facts come to hand which will entirely revolutionise our present ideas. So much the better. Science is so rapidly advancing that specialists in any branch have all their work cut out to keep pace with the times, and I look forward with confidence to the advance this phase of our study is sure to make in a short time.

One thing appears, however, quite certain. Whatever suggestions and theories may be developed, the process of "natural selection" will always have to be taken into account in the perfection of varieties and local races, whatever may finally be proven relative to the actual agents in the first production of such forms.

In conclusion, I can only say to my readers, that what I have written on this subject has grown from a comparatively short paper prepared a considerable time ago. The study of variation is increasing at such a rate, and there are so many good workers in the field that it would have been no trouble to have extended this contribution to the subject much beyond its present limits. However, enough has been written, and sufficient facts collected, I trust, for other workers to use, and it is to be hoped that the careful study of the subject may lead others into the field, to add to our store of knowledge. There are so many probable causes to be worked out in a subject of this kind that one is apt to emphasise some one special condition to the exclusion of all others. Perhaps I have erred in this, but I am in most hearty sympathy with Lord Walsingham's statement to the Fellows of the Entomological Society (*Trans. Ent. Soc. of London*, 1890, p. lv.), where he says :—"It cannot be too freely admitted that in all cases of supposed "natural selection," accompanied by advantage to the species, such advantage is probably by no means the sole and exclusive cause of, or inducement to selection—all the special

conditions under which the species exists must be taken into consideration, and any inclination to overrate the active value of one special condition should be carefully discounted. The study of such supposed causes and effects is yet in its infancy, and although the promising child has 'grown apace' under the loving care of its numerous admirers, it has by no means arrived at maturity; on the other hand, no jealous or disparaging critic can at present be justified in putting it down as an 'ill weed.'"

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